THE MANKIND QUARTERLY

VOLUME XXXVIII No. 4 Summer 1998

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Essays on Germanic Religion

EDGAR C. POLOMÉ

University of Texas, Austin

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THE INDO-EUROPEAN COMPONENT IN GERMANIC RELIGION

SOME COMMENTS ON *Vǫluspá*, STANZAS 17-18

DIVINE NAMES IN INDO-EUROPEAN

GERMANIC RELIGION: AN INTRODUCTION

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Cover Picture: Reverse side of a copper coin of the Yadheya Gana, 3rd/4th Century A.D., (*See Article:* The Mystery of the *Kalasa*: A Link to *Bharat Natayam*?)

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Thalamic Inhibition in the Evolution of Human Intelligence: Evolutionary Pressure for Cortical Inhibition

K. K. Glendenning[1]
Florida State University

One characteristic of a civilized society is the ability and propensity of its members to refrain from acting in a way that disrupts the harmony of the group. While this is not intelligence as measured by IQ, it is a form of social intelligence that will aid those having it in their ability to propagate. This characteristic is not unique to human societies but rather is true of most social species.

This report deals with one aspect of brain chemistry that may underlie this social intelligence, defined as the ability of a species to inhibit its thoughts as well as its behaviors. Selective pressure has unrelenting consequences on the mechanisms of brain function, and perhaps the first mechanism to be affected is the chemistry of the brain. Changes in brain chemistry and function can be explored by an investigation of species selected to represent the phylogenetic sequence leading to mankind. In an examination of nine species selected on this basis, we have found that one consequence of selective pressure is an increase in the potential for cortical inhibition. We suggest this may be a major component of the evolution of human intelligence.

Key words: intelligence, thalamus, inhibition, comparative psychology, evolution, mammals, receptor binding, GABA, muscimol binding, amniotic vertebrates.

By treating relative brain size as a measure of intelligence, it is possible to develop a coherent story about the probable history of intelligence as a biological phenomenon. Like other biological processes, intelligence must have evolved under the influence of natural selection. That is, a well known prime result of evolutionary pressure has been the selection for human intelligence.

The intelligence of humans has been shown to be correlated with an increase in size of the neocortex (eg., Ariens Kappers 1929; Diamond and Hall 1969). In this investigation we have extended this

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axiom one step further. Not only do humans have a larger cortex but we have found that humans also come equipped with an increased capability of inhibiting this expanded cortex.

To begin this investigation we began with the assumption that a look at our ancestral heritage gives us insight as to what the future may bring. In turn, an even broader spectrum of evolutionary analysis can lead to a similar insight and perhaps predictions about the path along which natural selection is taking us. That is, what lies ahead in the evolution of mankind is only a subpart of what has been happening to primates as they are a subpart of what has been happening to mammals and so on. By an examination of diverse species chosen to represent a phylogenetic series leading to man, this report shows one step in the consequences of selective pressure on the mechanisms of brain function. Our conclusion is that this step, an increase in the potential for inhibition of the cortex, is probably a major component of the evolution of human intelligence.

To draw this conclusion we must first examine the results of selective pressure on brain development. Secondary and tertiary synaptic levels in brain structure are relatively immune to changes in a species ecological niche. Natural selection first acts upon those parts of the brain most easily changed so they can revert with changing circumstances. For example, a species driven into a nocturnal habitat is subject to adaptive pressure on its visual system to adapt. The first noticeable gross anatomical changes are the receptors in the eyeball (Polyak 1957). Prior to these anatomical changes are changes in the chemistry of the brain. These are the most easily changed and therefore the first to be effected by selective pressure.

Yet the chemistry of the brain demonstrates a remarkable robustness throughout phylogeny. In this study we have looked at the constancy of an inhibitory neurotransmitter in the brain over a wide range of species. Particular attention has been paid to a part of the brain, the dorsal thalamus, through which all sensory information is channeled before it reaches the cortex.

With the advent of modern neurochemical analysis, the notion of the dorsal thalamus as a 'relay' of sensory information to the cortex has given way to the notion that it is a 'gateway' to cortex. In this conception, neural activity directed to the forebrain is actively

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modulated by a variety of inhibitory feedback loops.

An evolutionary dimension to the intrinsic modulation by the dorsal thalamus can be traced to Ramon y Cajal (1909, 1966) who proposed that the number of interneurons in the dorsal thalamus increased during the course of the evolution of mammals. In man, then, our greater intellectual capacity may not be just a function of our cortex but also of our dorsal thalamus. Further, it seems that the dorsal thalamus gates the information getting to the cortex by inhibiting much of it. The idea that the interneurons in the dorsal thalamus are usually inhibitory has been demonstrated in studies showing that most neurons are g-aminodecarboxylase (GAD)-immunoreactive and that more GAD-immunoreactive interneurons are present in species 'higher' in an evolutionary sequence leading to humans (Penny et al.1983,1984).

In the present study, the idea of an increasing amount of g-aminobutyric acid-A (GABA-A) inhibition over the Anthropoid ancestral lineage was investigated using seven mammals and two non-mammals selected to yield at least 5 differing grades or levels of recency of last common ancestor with Anthropoids. Any commonality among these would be relevant to the prototype of our common ancestor. Moreover, any change in pattern could be used to predict the direction these selection factors will have on the evolution of brain chemistry.

These species, each subjected to exactly the same receptor-binding procedure, were compared for the relative distribution of muscimol bound GABA-A receptors among the four subdivisions of the diencephalon.

The results support the view that the presence of g-aminobutyric acid and its role in inhibiting neurons in the dorsal thalamus is probably a fundamental characteristic of amniotes changing only in amount during the later evolution of mammals. This has implications for an increasing role of inhibitory synapses in mammals, in primates, and perhaps most importantly to us, in humans.

Materials and Methods

The labeling of high-affinity GABA-A receptors was accomplished by conventional receptor binding methods.(eg, Glendenning and Baker 1988, Cole et al.,1984, Nauta and Karten

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1970, Penney et al., 1981). Tritiated muscimol was used as the labeling ligand and GABA itself was used as the displacing agent for matched sections of control tissue (e.g., Snodgrass 1978)

For controls, myelin-stained material and binding of two other receptors (serotonin and muscarinic acetylcholine (ACh) was also performed to be sure that the pattern of muscimol binding among the diencephalic subdivisions was not merely the secondary result of differences in cell packing, neuropil density or total available membrane.

Sample Species

In all, 26 individuals from the 9 species were examined. Species were selected mainly on paleontological, taxonomic, and comparative anatomical grounds for their sequential recency of last common ancestry with Anthropoids (Fig. 1). This selection allowed the sample to contain mammals with at least 4 grades of successive kinship with Anthropoids: preplacental, two preprimate, and primate levels (e.g., Cronin and Sarich 1980, Dene et al., 1980, Luckett 1980, McKenna 1975, Szalay and Drawhorn 1980).

Therefore, the seven species of mammals which could be arranged on the basis of the remoteness of their last common ancestor with Anthropoids included Monodelphis and Didelphis opossums (Monodelphis domesticus, Didelphis virginiana), armadillo (Dasypus novemcinctus), rat (Rattus rattus), tree shrew (Tupaia glis), bushbaby (Galago senegalensis), and lemur (Lemur fulvus; eg., Nudo and Masterton 1988).

To these seven mammalian species, two non-mammalian amniotes with unarguably remote common ancestry with Anthropoids were added to the sample: a reptile (Iguana iguana) and a bird (pigeon, Columba livia). Together, the total of nine species in the sample could be used to deduce at least 5 phylogenetic grades (numbered 0 through 4 in Fig. 1).

Measurement of Receptor Binding

Because of the wide variation in the dissociation constant (KD) for muscimol-binding, we performed standard statistical analyses of variance of the dissociation constants in 71 reports published over a 12 year period. Separate analyses, using 'laboratory

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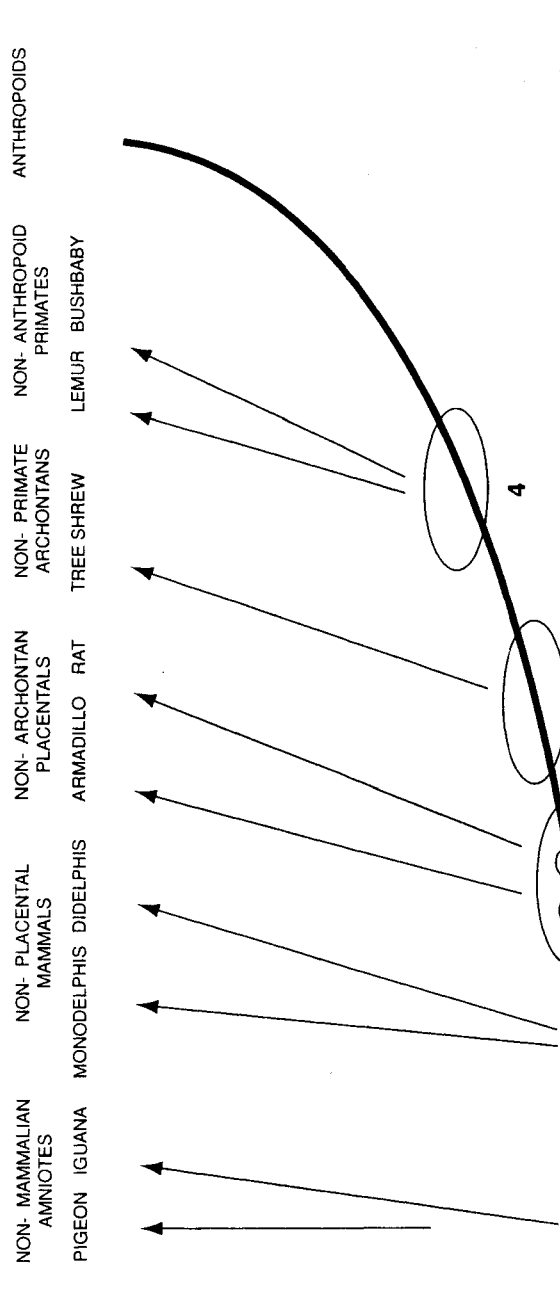

Figure 1. Relationship of the nine amniotes in the present sample with five successive hypothetical ancestors in the Anthropoid lineage. Premammalian (0), mammalian (1), placental (2A,2B), archontan (3; Luckett, 1980), and primate (4) ancestral grades are arbitrarily labeled to yield a total of 5 distinguishable grades. Note that if edentates (2A; Armadillos) are accepted as having more remote common ancestry with Anthropoids than rodents (2B; e.g., Novacek 1992) then the 'last common ancestor' scale would expand to a total of 6 separable grades, with 4 grades within placental mammals alone.

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of origin', 'tissue procedure', and 'species' as single factors showed that it was 'laboratory' alone which was the significant factor in the variation of KD values ($p < 0.0001$). The other two factors, 'tissue-procedure' and 'species' singly or with interactions yielded no significant variation (tissue-procedure, $p > 0.7$; species, $p > 0.8$; tissue-procedure x species, $p > 0.8$).

Therefore, we chose to standardize the procedure to be certain that the comparison of muscimol binding among the present species would not vary as a result of even minor variations. Consequently, the tissues from all 26 individuals in the 9 species were treated identically including exposure to exactly the same concentration of ligand.

The in vitro receptor-binding technique followed the protocol of Kuhar(1985) and his colleagues (Nauta and Karten 1970, Zarbin et al., 1981 and see Beaumont et al., 1978, Glendenning and Baker 1988, Shaw et al.,1984). For the control binding of serotonin receptors with serotonin and muscarinic ACh receptors with quinuclidinyl benzilate (QNB), the procedures of Pazos and Palacios (1985) and Yamamura and Snyder (1974), respectively, were followed. The individuals were routinely euthanized following the guidelines of the AVMA Panel on Euthanasia (1993).

Microdensitometry of the Tritium-sensitive Film

For each procedure, the sections were exposed to tritium-sensitive film along with autoradiographic tritium microscales which were placed with the sections in the film-cassette (Geary et al.,1985). These microscales allowed the optical density of the autoradiographic images produced by the bound ligand to be converted into relative densities.

Microdensitometric analysis was accomplished using a microcomputer imaging system (Imaging Research, Inc.). Following calibration, a 'total' binding image was digitized and then overlaid with the 'non-specific' binding image. The 'specific' binding image was the difference between them. The tissue for analysis, i.e., dorsal thalamus, epithalamus, etc., was outlined on the 'specific' binding image of the thalamus and average relative optical densities were generated for each subdivision. The values shown in the tables and graphs are the averages of at least twelve photo-density measure-

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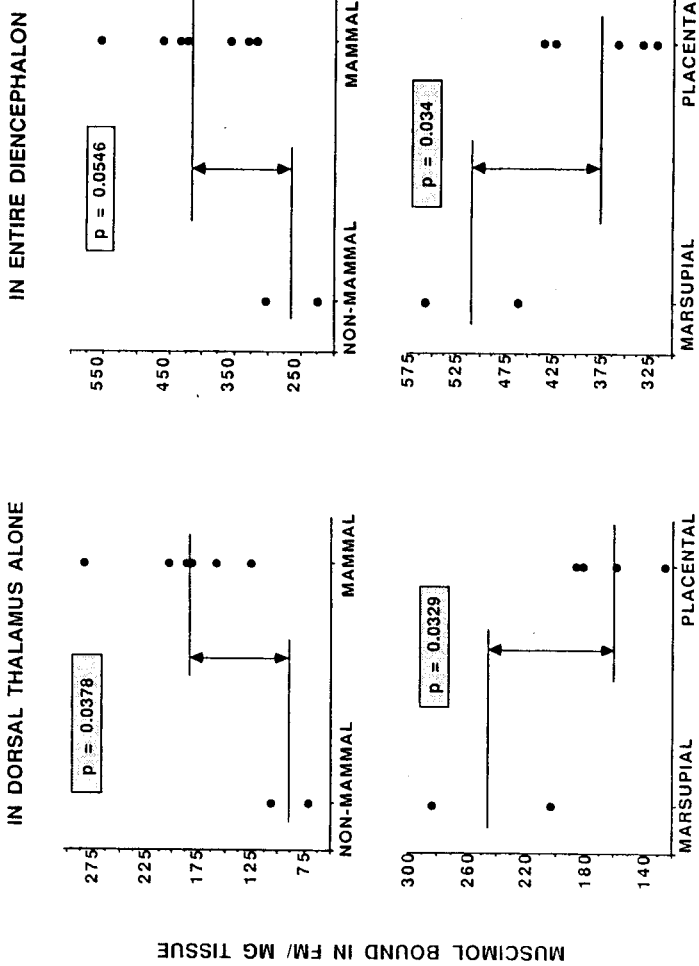

Figure 2. Four scatterplots of the distribution of bound muscimol in dorsal thalamus (left) or entire diencephalon (right). Top graphs compare non-mammals and mammals. The difference between non-mammals and mammals is statistically reliable only for dorsal thalamus (see gray p-value in top left). Bottom graphs compare marsupials and placentals. The difference between marsupials and placentals is statistically reliable both for the dorsal thalamus (bottom, left) and for the entire diencephalon (bottom, right).

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Muscimol specific-binding (femtomoles / mg tissue)

Animals	Epithalamus	Dorsal Thalamus	Ventral Thalamus	Hypothalamus	Diencephelic Mean
Iguana	46	70	40	67	56
Pigeon	46	106	64	86	76
Monodelphis	57	283	88	127	139
Didelphis	49	203	80	128	115
Armadillo	40	125	56	96	79
Rat	49	158	63	87	82
Tree Shrew	52	158	93	130	108
Bushbaby	46	181	62	133	106
Lemur	36	186	47	62	83
Non-Mammalian Mean	46	88	52	77	66
Mammalian Mean	47	185	70	106	104
Marsupial Mean	53	243	84	128	127
Placental Mean	45	162	64	102	92
Primate Mean	41	184	55	98	95

TABLE I. Specific muscimol binding (total binding minus nonspecific binding) in the four subdivisions and the total diencephalon for the seven mammals and two non-mammals. In each case the greatest degree of binding was found in the dorsal thalamus.

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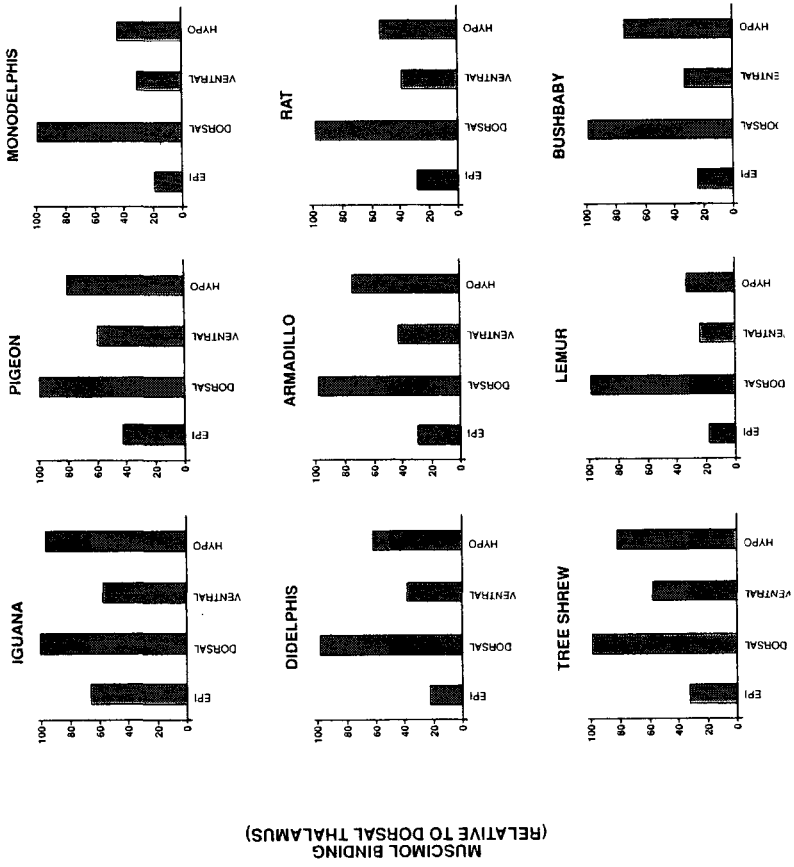

Figure 3. Muscimol specific-binding in the subdivisions of the diencephalon for the seven mammals and two non-mammals relative to the binding in dorsal thalamus. Note that the binding in the dorsal thalamus is the most dense in each species while the binding in the hypothalamus is more dense than in either the epithalamus or ventral thalamus.

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ments of each subdivision, six from each side of the brain.

Results

There are three chief results: 1) inhibitory receptors as shown by muscimol binding was far denser in the dorsal thalamus than in any other of the four major subdivisions of the thalamus for each of the 9 species; 2) the pattern of muscimol binding across the four thalamic subdivisions was similar regardless of the species' taxonomic class, subclass, order, or family; and 3) only the dorsal thalamus in the placental mammals suggested a systematic increase in muscimol binding over geological time. Since it is also known that phylogeny brings an increase in the development of cortex, the systematic increase in inhibitory receptors in the dorsal thalamus of placental mammals is correlated with the increase in the development of the cortex of placental mammals.

Muscimol Binding

In each of the seven mammals, a broad band of high-density binding was confined to dorsal thalamus.

The microdensitometric measurements of relative muscimol binding in each of the four subdivisions of the diencephalon is tabulated in Table I. It can be seen that the density of binding in dorsal thalamus always exceeded that in the other diencephalic subdivisions. It should be noted that there was also a relatively high density of bound muscimol in the dorsal thalamus of the non-mammals, although it was less dense than in the mammals (compare scaling of vertical axes in Fig. 2). Table I also shows that there was more bound muscimol in the entire diencephalon of the mammals than in that of the non-mammals.

The profiles of the relative density of muscimol binding across the 4 subdivisions, with dorsal thalamus arbitrarily standardized to 100%, are illustrated in Figure 3. It can be seen that the density of muscimol binding in the dorsal thalamus (the second bar of each graph) is far higher than the other three subdivisions of the diencephalon while binding in the hypothalamus is higher than in either epithalamus or ventral thalamus.

Although there was variation in binding among the several species of mammals, the relative density among subdivisions of the

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diencephalon was similar within each species: a dorsal to ventral, low-high-low-high profile was obtained regardless of species. Therefore, this generalized profile of bound muscimol across the diencephalic subdivisions may be a common characteristic of amniotic vertebrates, whether mammalian or not.

It should be noted that in order to produce comparable profiles of relative binding density among the species, the scale of optical density differs among the species (compare entries in Table I). This scaling, showing relative optical density rather than absolute numbers of receptors, allows comparison among the species despite possibly differing dissociation constants for muscimol.

Comparison of Profiles of Muscimol Binding with Other Ligands

Before pursuing some possible factors leading to the variation in levels of muscimol binding, it is noteworthy that two other receptor binding ligands did not show the same general pattern as that of bound muscimol.

That is, while the dorsal thalamus is heavily bound with muscimol, it is relatively free of 5-HT binding. Other data leading to the same conclusion were found in the binding of the other control ligand: QNB binding of mACh receptors also does not parallel the profile of muscimol binding. Taken together these two controls make it unlikely that the muscimol profile across the thalamus is based solely on the differences among the subdivisions in cell or neuropil density.

Therefore, the heavy muscimol binding in the dorsal thalamus probably is not the secondary result of differences in cell or neuropil density nor in total available membrane.

Taxonomic Differences in Muscimol Binding

Returning to the muscimol binding results themselves, Figure 2 allows comparison of the relative amount of bound muscimol in the dorsal thalamus and in the entire diencephalon, for non-mammalian vs. mammalian species and for marsupial vs. placental mammals. In the top graphs it can be seen that a greater density of muscimol binding was found in the mammals than in the non-mammals in dorsal thalamus ($F61$, $p < .04$).

In the bottom graphs in Figure 2, it can be seen that the marsupials (Monodelphis and Didelphis opossum) had relatively more

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Figure 4. Specific muscimol binding for the nine animals arranged on the basis of recency of their last common ancestor with mankind. Three lines in each graph show relationships of binding in successively smaller subsets: amniotes (n=9), mammals (n=7), and the placental mammals (n=5). Only the relationship between muscimol binding in the dorsal thalamus among the five placental mammals is statistically reliable (see shaded label in the left graph).

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muscimol binding than the placental mammals and this statistically reliable difference appeared both in the dorsal thalamus alone (F_{61}, $p < .04$) and in the entire diencephalon (F_{61}, $p < .04$).

Possible Phylogenetic Trends in Muscimol Binding

Because reliable mammal/non-mammal differences and reliable marsupial/placental differences were found in the relative density of muscimol binding in dorsal thalamus, we were encouraged to analyze the possibility of a phylogenetic trend along the Anthropoid ancestral lineage. Figure 4 shows the amount of muscimol binding among the species when they are arranged on the basis of their nearness of kinship, or recency of last common ancestor, with Anthropoids and thus with humans (i.e., see Fig. 1).

It can be seen that for the entire group of species, no reliable trend appears (see dotted line in Fig. 4, left). Nor does a trend appear when the two non-mammals are omitted and only the seven mammals included (see dashed line in Fig. 4, left). It is only when the two marsupials as well as the two non-mammals are omitted (leaving the 5 placental mammals alone) that a statistically reliable trend in muscimol binding appears in dorsal thalamus (see solid line in Fig. 4, left). Therefore, despite the small sample size of placental species ($n = 5$) and with only 3 deducible grades (grades '2', '3', and '4'), an increase in muscimol binding along the placental segment of the Anthropoid ancestral lineage is probably present. There is no reason to think that this would not continue into the human lineage.

Discussion

The primary results show that profiles of the relative density of muscimol binding across the four major subdivisions of the thalamus are similar among amniotic vertebrates regardless of their class, order or family. In contrast, the results suggest that there are statistically reliable differences in muscimol binding between mammals and non-mammals, between marsupial and placental mammals, and depending on their degree of kinship with Anthropoids, among the placental mammals in the sample.

The Neurochemical Plan of Amniote and Mammalian Thalamus

The constancy of the relatively dense muscimol binding in the

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dorsal thalamus across the nine diverse species of amniotes suggests that the presence of high affinity GABA-A receptors in the dorsal thalamus may be a fundamental principal of neural organization at least among later vertebrates. As noted above, similarity of particular receptor types across a variety of species, orders, classes, and even phyla has been noted by others and has obvious implications regarding neurochemical 'conservation' over long periods of brain evolution (eg., Cole et al., 1984, Mann and Enna 1980, Nielsen et al., 1978). One is inclined to think of directional selection tending to alter a species, rather than to keep it constant. In point of fact, stabilizing selection is probably usually maintaining the constancy of a character rather than altering it (i.e. see Sheppard, 1960). This seems to be true even at the level of the biochemistry of the synapses.

Second, the increase of muscimol binding in mammals compared to non-mammals suggests an increase in high-affinity GABA-A receptors in mammals and therefore, a greater role of inhibitory processing in the mammalian variety of dorsal thalamus. Because there are close parallels in the evolutionary development of dorsal thalamus and cerebral neocortex, a difference in the thalamus in mammals (see hypothetical ancestor '1' in Fig. 1) and their premammalian ancestors ('0' in Fig. 1) might well be a concomitant of the enhanced cortical development of the cerebrum seen in the mammals over the same period of geological time -- perhaps a demand placed on the dorsal thalamus by the emergence of mammalian cortex itself.(e.g., Nauta and Karten 1970, Northcutt 1984, Pritz and Stritzel 1994, Rose and Woolsey 1949, Veenman et al., 1994, Butler, 1994). That may mean for human evolution there has been a progressive increase not only in cortex but also in the thalamus supporting the cortical development. Further, along with selective pressure for an increase in cortical size has come an increase in the inhibitory receptors and therefore the ability to inhibit ongoing activity.

Finally, the notion that the GABAergic gating in the thalamus might have been altered during the further evolution of the Primate (and therefore human) brain is generally supported by the results of the present study (cf, Cole et al.,1984, Mann and Enna 1980, Nielsen et al., 1978, Penny et al., 1984).

Since the dorsal thalamus eventually achieved a high density both

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of GABA-A receptors and GABA-ergic neurons in Primates (including humans), the dorsal thalamus seems to have had a unique and persistent status in its potential for inhibitory and probably disinhibitory gating of information directed toward the cerebral cortex. Perhaps this is the basis for the saying that the mark of intelligence lies as much in what one does not say or do as it does in one's words or actions!

Acknowledgements

I dedicate this manuscript to the late R. Bruce Masterton because it was in his continued search for links with our common ancestry that the seeds of this research were born. I also thank Bobbi Baker for her knowledge and skill with receptor binding techniques. This work was supported in part by NIH grant NS07726.

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Essays in Eugenics

Sir Francis Galton, F.R.S.
With Biographical Introduction by Roger Pearson

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Expanding the Boundaries of Evolutionary Psychology: The Context of Domain-Specific Adaptations

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The tide of evolution carries everything before it, thoughts no less than bodies, and persons no less than nations.
George Santayana

Evolutionary psychology relies heavily on domain-specific selection, positing that cognitive and emotional processes are adaptations for solving particular problems of survival and reproduction. This model ordinarily does not consider alternative mechanisms of evolution. It also restricts explanations for the biophilic nature of humans, and limits the appreciation for the profound effects of the environment on the origin and expression of human traits. Drawing heavily on classical and recent concepts of contemporary conditioning theory, a testable model is presented that broadens domain-specific selection to include the co-evolution of contextual stimuli ("contextual evolution"). It incorporates non-specific sensory regulation of evolved behavior, including biophilic regard for certain environments, and suggests what the evolutionary conditions might have been for the emergence of human mental complexity.

Key Words: Sociobiology, evolutionary psychology, domain-specific adaptation, evolutionary theory.

We are witness to a revolution in the understanding of life. From molecules to neurobehavior our system of reductionistic logic and methods of analysis confirm life's evolutionary origins and its genetic character (Boyd & Silk, 1997; Barkow, Comides & Tooby, 1992; Gazzaniga, 1995; Strickberger, 1996).

The reductionistic approach that has worked so well in the understanding of evolution has been embraced by evolutionary psychology in an attempt to elucidate the adaptive significance and species-specificity of human cognition and behavior. The irony of this

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species-specificity of human cognition and behavior. The irony of this approach is that the broader qualities of the human psyche, such as the awe and fear of nature, the non-specific motivations that often guide behavior, and the feelings of connectedness with the surrounding world and with other species, go unexamined. In exchange for reductionism, science has conjured up a sterile view of humans, one lacking morality, nobility, and principle—a dogged, selfish creature with little to recommend it—a protoplasmic bag of genetic tricks, buffeted back and forth by the environment until its death in an indifferent world. Life is seen as nothing more than DNA molecules racing madly toward reproduction and annihilation. Not a pretty picture.

In my opinion the current emphases within evolutionary psychology on genetic reductionism and reproduction constitute a restricted view of human behavior. It is not that I see evolution as something other than a mechanical process, but that the Darwinian and psychological approach are too narrow, emphasizing the struggle among genes for reproductive gain while forgetting that human evolution was an emergent process of adapting to the vast uniformities and the most basic nature of the surrounding universe (Lykken, McGue, Tellegen, & Bouchard, 1992). There is a larger world of natural selection that is overlooked – a selection by the continuous and cyclical environments (connecting us with the vast universe) – a genetic impression of physical and psychological surroundings into the DNA that imbues life with purpose, provides humans with a kinship to plants and animals, and initiates self-examination (Kellert & Wilson, 1993).

Modifying the Paradigm of Evolutionary Psychology

I have two goals in this essay. The first is to show the limited value of current notions of natural selection within evolutionary psychology to account for the more general and non-reproductive qualities of humans. The second is to propose a broader view of evolution and natural selection that might account for the distinctive nature of human behavior.

In pursuing these goals it became obvious that evolutionary psychology and classical stimulus-response (S-R) learning theory

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share many features in common.[2] It is also evident that learning theory may offer concepts of value in attempts to understand the complexity of human evolution. Evolutionary psychology is clearly S-R in character. Both paradigms assume a tight association between stimulus events and behavioral outcomes, one based on adaptive changes over generations and the other based on reinforced changes over trials. Strip away the differential emphases on genes or environment and the paradigms overlap in theoretical interest and experimental methodology (see B.F. Skinner, 1981, for an earlier discussion).

Indeed, the similarities between evolutionary psychology and classical S-R conditioning theory suggest that evolutionary psychology is following the same bumpy conceptual path laid down by learning theorists. One may be able to understand evolutionary psychology in terms of its historical overlap with conditioning theory. Of greater concern here is the possibility that recent advances in S-R theory, particularly the demonstrations of contextual effects on learning, can be applied effectively to evolutionary questions of human behavior.

This paper will pursue the analogy between S-R theory and evolutionary psychology, attempting to show that the contextual regulation of S-R connections which act as major modulators of learning may also be the type of environmental regulators that broadened the specificity of natural selection, giving humans an unusually large range of non-reproductive variation and correlated affective tone.

Current Notions of Domain-Specific Selection

The Darwinian view of the natural selection of behavior is that it is specific to the time and place of occurrence (Cronin, 1991; Maynard Smith, 1989). Selection is indifferent to future events. Slowly, with incremental accumulation of change, behavior adapts to environmental demands (Dawkins, 1996). The result is often a

[2] The term S-R learning is meant to refer to the general associative process postulated by E. Thorndike and later employed by C. Hull, K. Spence, and other neobehaviorists. S-R theorizing is not the basis for current theories of associative learning; that distinction belongs to Ivan Pavlov. Contemporary theory has, nevertheless, joined notions of both.

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collection of species-typical *modules* (read "instinct"), otherwise known as domain-specific adaptations (Tooby & Cosmides, 1992). According to this view there can be no selection for a character that is only valuable to solve future problems, a domain-general adaptation (however, see Bjorklund & Kipp, 1996, for a discussion of domain-general behaviors). Therefore, for example, there is low likelihood of the evolution of general processes of morality, spirituality, and intelligence ("g"). They must be built from more specific problem-solvers.

The modular nature of organisms has been likened to a Swiss army knife where each component has specific structures and functions. Organisms have specific tools for foraging, predatory defense, and mate choice; the Swiss army knife has precise tools for cutting, scraping, picking, and screwing, hence the analogy for modular specificity. Although knife tools and behavioral adaptations are specific to their evolutionary history, they can be used to solve novel problems: the historical specificity remains true to its original nature, though the utilitarian value of the adaptation changes. A knife blade can be used to perform an emergency tracheotomy, and a foraging strategy might be used to find a car in a parking lot. It is the application of specific adaptations to novel problems that gives the *appearance* of domain-general adaptations: the illusion is that we have general purpose mechanisms when in fact we simply apply specific strategies in novel situations.

The biological implications of this reasoning are these:

1. Natural selection results in domain-specific adaptations and underlying mental processes. Indeed, it is believed that the brain shows a comparable specificity of structure and function (Gazzaniga, 1995).
2. Behaviors and mental processes are relics of earlier adaptations, reflecting past rather than current imperatives. The natural selection processes is mechanical and indifferent to the future needs of individuals. What humans know of the world is a reflection of earlier adaptations to "solve" (survive) particular problems.
3. The specificity of selection prevents the evolution of domain-general behaviors and mental processes. Natural selection does not build mechanisms that allow organisms to anticipate environments

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not experienced. One cannot evolve a strategy in the absence of ecological pressures for that strategy. Broad strategies appear to be general and anticipatory but their structure can be decomposed into their domain-specific elements, such as with morality that can be broken down into situational advantages (Wilson, 1975), or with general intelligence that can be divided into individual components of ability (Cattell, 1971).

The growing theoretical influence in evolutionary psychology of domain-specific selection is in part responsible for the mechanistic view of human nature and ultimately the refusal to grant humans a broad and flexible understanding of their own condition. Evolution is seen as a process of refinement, rather than expansion – a repetitive stripping away of nonessentials as the environment ties individuals progressively to criteria of reproduction. Much like the learning process described by Harry Harlow (1959), evolution strips populations of errors (non-reproductive attributes) as genetic relations are forged between stimulus events and responses (reproduction). The end product is specificity of physiological and behavioral adaptations.

The Failure of Domain-Specific Selection: History Revisited

Domain-specific selection is a compelling extension of the mechanistic rationale of natural selection to all aspects of behavior and human cognition. It leaves little to chance, non-adaptive evolution, or general process selection. Much like learning theory in the first half of this century, natural selection theory is an associationistic stimulus-response paradigm, the specificity of environmental pressures resulting in particular responses with functional value. Its appeal is in its simplicity of relating all organismic responses to reproduction.

Classical S-R learning theory was ultimately found to be incomplete for explaining the complexity of behavior, which should be a lesson for biologists seeking equal situational specificity. The S-R model was too specific and self-contained. It did not reflect general events in the learning process, it did not account for individual differences in performance, nor did it give credence to phylogenetic constraints on learning or general cognitive processes

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(Domjan, 1983; Roitblat, Bever & Terrace, 1984; Seligman, 1970). Animals proved to be too complex to be understood in terms of linear links between stimuli and responses.

The academic S-R movement was nevertheless a very powerful approach to the understanding of simple behaviors. The notion was simply cast too widely, advocated too strongly, and defended too vigorously by its originators (Amsel, 1972; Hull, 1930; Spence, 1936).

Evolutionary psychology, with its view of specificity of function and its singular reliance on natural selection, seems destined to commit the same errors as did theorists of learning. Evolutionary psychology is clearly S-R in character: it relies on the specificity of linear connections between organismic characteristics and reproductive success; it is self-contained in its analysis, omitting non-adaptive and general processes of behavioral control, and it is defended vigorously (Dawkins, 1996 ; Dennett, 1995; Symons, 1992). Much like S-R learning theory, as originally developed, evolutionary psychology may successfully explain simple behaviors, but falters in the unraveling of the deeper desires and fears of human behaviors. A sketchy comparison between S-R learning theory and natural selection theory of evolutionary psychology is given in the following table, revealing the overlaps.

The most irresistible aspect of both classical S-R theory and its equivalent adaptationist model of Darwinism is that all behavioral designs, however complex, are built through a simple repetitive process (Dennett, 1995; Hull, 1930; Maynard Smith, 1989). One needs to know little about a behavior: there is no requirement to know the origin or development of a behavior, as the historical process of behavioral reinforcement or of genetic adaptation is assumed from the complexity of the behavior and its utilitarian nature (but see Gottlieb, 1992; Raff, 1996). Moreover, the models are powerful predictors of environmental and genetic change. Any observer can see the processes at work, as animals adjust behaviorally to reinforcement or change genetically in face of ecological selection pressures.

The troublesome aspects of the classical S-R and current adaptationist paradigms are that they essentially disregard the phylogenetic and developmental history of behaviors; they ignore alternative explanations of behavioral change; they have little to say

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about the function of individual and group differences; and they assume the identical incremental selection of adaptive responses for all traits. The very power of the logic to explain everything using the same reiterative process leaves one gasping at its simplicity and generalizability. The models explain much, but ultimately fail because of their inability to discriminate between alternative mechanisms of behavioral control, their underestimation of extraneous influences, and their thin concern for tracing the developmental and phylogenetic factors leading to response patterns.

Contextual Learning and its Survival Value

In my opinion classical learning theory was saved from self-destruction by its recognition that phylogenetic constraints sharply modify the nature of the S-R connections (Breland & Breland, 1961; Garcia, Hankins, & Rusiniak, 1974) and by its attention to the regulatory nature of the context in which S-R connections are formed (Balsam & Tomie, 1985; Bouton, 1993; Overton, 1964; see Klein, 1991 for a general discussion). It is the context, in particular, that expands the significance of the classical conditioning paradigm. Learning theory also offers a theoretical view which, when reduced to individual differences in sensitivity to the surrounding environment, can account for the diverse nature of domain-specific selection. I will first look at the nature of contextual learning and in the following section apply this notion to the evolution of human behavior.

The S-R nature of learning is not independent of its environment. Associations occur within a broader context, linking the specificity of the learned behavior to a more general influence of its surroundings. The context of the learning reinforces, limits, and canalizes the learned behavior. Thus a learned avoidance of a nighttime snarl from a nocturnal cat is contingent on darkness and may never be displayed in another context. Later the darkness may come to evoke fear reactions without regard to the specific association of snarl and avoidance. The magnitude of the fear may be enhanced or degraded by the many other stimulus-response reactions that occur in the same or different contexts. What began as specific conditioned associations becomes a more general biophobic reaction to darkness.

The point is that organisms link specific cues to responses but

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Table 1

Conceptual Overlap Between Classical S-R Conditioning Theory and Natural Selection Theory

Area of Overlap	S-R Conditioning Theory	Natural Selection Theory
Historical Origins	Ivan Pavlov, Edward Thorndike	Charles Darwin, Alfred Wallace
Specificity of Organism-environment Connection	Stimulus event followed by response (S-R specificity)	Environment specifying selected adaptations (domain-specific selection)
Crystallization of Paradigm	Clark Hull, Kenneth Spence,	Richard Dawkins, John Maynard Smith, Leda Cosmides, John Tooby
Unit of Behavior	Unconditioned response (instinct)	Module (instinct or fixed action pattern)

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Measure of Success	Specific S-R connections leading to reinforcement (cue to consequences association)	Specific environment-response adaptations (cue to consequences association)
Response to Novelty	Applicability of original S-R relations to new situations (generalization)	Application of old modules to new situations (generalization)
Refinement of Behavior	Elimination of errors over trials (H. Harlow's error learning; B.F. Skinner's shaping)	Elimination of non-adaptive responses over generations
Inhibition of Behavior	Extinction of S-R connections (extinction & active inhibitory processes)	Failure of adaptations (genetic extinction & new adaptations)
Restriction of Theory	Exclusion of phylogenetic constraints, general cognitive processes, and chance	Exclusion of phylogenetic constraints, domain-general processes, and chance

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they also incorporate contextual stimuli into their associations. Learning theorist Mike Domjan (1997) concludes that, "Not only discrete stimuli, but also background contextual cues, can come to control behavior. Furthermore, stimulus control by contextual cues can develop even if attention to contextual cues is not required to optimize reinforcement." (p 382) A closer view of learning will highlight the more general features of the regulation of S-R relations.

Discrete discriminative learning always occurs in the presence of background contextual stimuli. These stimuli which the organism is not required to notice nevertheless take on cue value within which S-R connections are made. For example, in one experiment by Balsam (1982) pigeons were conditioned to expect periodic food reinforcement (variable interval conditioning) in a noisy chamber but never in a quiet chamber. The pigeons soon showed increased locomotion when placed in the noisy chamber but not when placed in the quiet chamber. While noise did not signal exactly when food would be provided, it nevertheless acquired modifying influences on the animals' behavior.

Rescorla, Durlach, and Grau (1985) increased the specificity of contextual learning by conditioning pigeons to peck a key when the key was illuminated with an orange light. A noise was used as a modifier of the S-R connection for half the birds and a diffuse flashing light was used in place of the noise for the other half of the birds. The results of the conditioning procedure again showed the modulator effect of background noise or light. The birds pecked the orange cue much more when it was presented in conjunction with the modulator cue than when it was presented in isolation. Note that the background noise or light were never contingencies for reinforcement of pecking yet they became facilitators of the response simply because they occurred in the same setting as the reinforced trials.

These and several similar experiments clearly show that S-R relations are in large measure dependent on background events. Context modulates the acquisition and maintenance of a conditioned response, and when different, the response is degraded or is eliminated altogether. Investigators such as Rescorla (1985) have made the important point that even when the conditioned response to a specific stimulus is extinguished by non-reinforcement, the animal's reaction to the background stimuli is not effected. In other

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words, once a modifier cue gains control over an S-R relation, it takes on a life of its own in the regulation of behavior. Theoretically, an animal can demonstrate either appetitive (approach) or aversive (withdrawal) behavior long after the S-R relation to the original reinforcement is extinguished. The contextual stimuli emerge as "biophilic residuals" (emotional traces) long after the link between stimuli and responses have been disassociated.

The background context of learning is important for another reason. Once a response is learned it can more easily be retrieved in the presence of the contextual cues. Bouton (1993) conditioned an infant's kicking response in a playpen that was either striped or had a square pattern. The short of it is that the retention of the performance was significantly improved under conditions where the playpen liner was the same as that occurring during acquisition of the behavior. Thus, memory is in part a function of the appropriate contextual cues, similar to those described as state-dependent learning and memory (Klein, 1991; Overton, 1964). According to Thomas McKelvie, and Mah (1985) organisms may never "forget" a learned behavior; rather, they simply learn new relations. Which memory is retrieved depends on the retrieval (contextual) cues present during the attempts to remember. If an S-R association is conditioned (reinforced) in one context and extinguished (non-reinforced) in another, returning the animal to the original training context will reactivate the memory of original conditioning. Presumably one could demonstrate a wide array of behaviors and associated emotional reactions to a combination of previous S-R experiences. The variations in context determine the form and intensity of the behavior.

Contextual modification of S-R conditioning broadens exponentially the behavioral significance of learning. Individuals not only learn the critical links between stimuli and behavior, they express or suppress those links to the degree that the original context is duplicated. Survival may turn on this sensitivity. For example, foraging for and the eating of fruits may occur successfully in a context of seasonal rain. Later episodes of rain induce the retrieval of the stimulus-response links in finding and eating fruit, facilitating those behaviors. Similarly, the success of finding appropriate mates, avoiding predators, and a host of other survival behaviors, may

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correlate with the contextual information associated with these events. The same S-R connection, such as food searches and eating, may vary in adaptive ways depending on the variable backgrounds in which they occur. Rarely does a behavior occur in only one environment; it occurs in many, each of which may add to or subtract from the organism's information and survival. The context increases or decreases the probability of a learned behavior and provides the emotional tone of the act.

Contextual Evolution and Variability of Specific Adaptations

Contextual cues guide the expression of an evolved response, as they do with S-R conditioning. We see the variable results of a behavior that hinge on context. An animal pursues a "dove" or "hawk" strategy according to such things as age, sex, season of the year, territorial status, body size, and the behavior of its competitors (Maynard Smith, 1974). Similarly, anti-predatory behaviors, courtship rituals, parental investments, and foraging strategies depend heavily on the environmental and social context of these acts. The general principle seems to be that the animal is behaviorally blind without the facilitating or inhibiting context of the environment, much as a frog is sensorially blind to a fly that does not move (Maturana, Lettvin, McCullock & Pitts, 1960).

It is unlikely that there was natural selection for specificity of function within every environment in which a behavior occurs. There are simply too many environments and too many variation of behavior. Animals did not evolve multiple forms of dovishness of hawkishness to suit each and every possible condition. It is more probable that the basic attributes of vigilance and aggression were modified in form and intensity according to the animals' surroundings. There are no different modules for dovishness or hawkishness, only a single response system regulated in a near infinite fashion, reflecting the degree to which the evoking environment mimics the environment in which the original behavior evolved.

The distinction of what I call "contextual evolution" from the domain-specific selection process is substantial, for it suggests that there was limited evolutionary specificity for complex behaviors. Complexity occurs in part because there is broad behavioral genetic expression of behavior correlated with the animals' surroundings

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during evolution. This contextual modification is also specific – specific to the context – but it is highly variable and not an invariant part of the evolution for reproductive traits. The variability of a response lies not in the multitude of specific forms in which it evolved, but in the degree to which a particular environment is similar to the original environment in which it was originally selected. For example, if the response was selected first in environment A, its expression will vary to the degree that a new environment is similar to A. The advantages of this postulate are several.

1. It is not necessary to assume that all variation in human behavior reflects variation of natural selection for adaptive behaviors, however complex the behavior may be. The variation stems from environmental context that modulates evolved responses.

2. One can dispense with the notion that complex behaviors necessarily reflect a long history of incremental selection for adaptive behaviors. Behaviors evolve for many reasons (pleitropy, drift, allometry, self-organization, or adaptation), but once evolved they vary in expression according to the surroundings of their origins. Much of behavioral complexity may stem from this.

3. No environment, other than the original context, is the "natural" environment for that behavior. Because an animal's situations always are changing, it follows that virtually no environment ever duplicates the original environment. Thus, nearly every environment, including the technological surroundings of man, is just another variation in context that will modify the form and intensity of the behavior. In that sense there is no more reason for suggesting that we are mismatched with today's environment any more than any other previous environment.

4. As with S-R conditioning, the context of behavioral acquisition provides the emotional tone of the behavior, its intensity, and the form of its expression. Hollingworth as early as 1928 captured the relation between context and behavior in his description of the acquisition of a snake phobia: "In the first place the "appearance of the snake" is complex. The snake itself is an elaborate pattern of occurrences – color, shape, size, movement. Moreover, the "appearance" implies the presence of a further background from which this pattern emerges. And

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there are adjacent objects, the time, place, and other attendant circumstances. The fear is not of the snake but of the total situation and circumstances." (p 375)

5. The context may be able to evoke the emotional quality associated with the original acquisition without triggering the specific behaviors, just as contextual learning can be disassociated from specific S-R conditioning. It would depend on how readily the contextual cues are disassociated from the original evolved response. If disassociation is possible humans may be able to experience a rich world of pleasure and pain independently from the initial evolution for traits of reproduction. The contextual patterning of an organism's existence then becomes the driving force of behavior and emotional life. It would seem that the context of evolution is highly variable and the emotional tags associated with the behavior and its surrounds are equally broad and impossible to ascribe to a single environmental condition.

6. One need not postulate that reproductive success is the criterion for expression of a behavior in a new environment, or even that the behavior ever had that relation to reproduction. That logic should be inverted. An organism that evolved a behavior in one environment is more likely to show that behavior, including reproduction, to the degree that the new environment mimics the original environment. Reproduction need not be the organizer of behavioral designs, but rather the consequence of an organism preferring and recapturing its original context. What an organism may do is pursue contextual cues that match the earlier selection history. Once a great enough similarity is found the organism may display the associated reproductive behavior.

7. The proposed model is in no way Larmarkian. The ecological context of an evolutionary change is not impressed into the DNA through simple exposure or learning. The contextual nature of the surrounding environment is part of the evolved behavior – a co-evolution between the event stipulating a change in reproductive value and the qualities of the surrounding environment. An organism that incorporates the contextual configuration into a differential DNA change may survive more readily and reproduce when the original context reappears.

Genetic change resulting from a surrounding environment

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probably depends on individual differences in the sensitivity to the environment associated with a primary change in DNA related to reproduction. An organism whose behavior is regulated by the context may have a selective advantage over another whose behavior is not regulated. Thus, an animal who can associate rain with successful foraging may prevail over another who cannot. What is connected is not the direct association between rain and reproduction but the indirect association of rain with the more primary association of food and survival. The context of rain rounds out the acts leading to survival and gene replication.

The assumptions are that the regulatory features of the context are more variable than the more primary response evolved, that they rest on individual differences in sensory sensitivity to backgrounds, and that they are easily linked or disassociated from major evolutionary changes. Again, the associations between foraging skills and reproduction are relatively invariant, but the regulation of their expression may shift as the surrounding conditions change, genetically favoring individuals with sensitivity to background conditions.

Indirect and often unrecognized connections between evolution and environmental context allow us to speculate why humans seem to harbor not only the response patterns once linked to reproduction but also the emotional feelings of agony and ecstasy fired by the context of their evolved nature (Kellert,1997; Kellert & Wilson,1993).

The Future of Evolutionary Psychology

Evolutionary psychology recently diverged from sociobiology by offering evolutionary interpretations of human cognition and behavior (Barkow, et al, 1992). Its interpretative reach is extensive, especially in the area of mating strategies, where evolutionary psychology moved rapidly from abstract theory to empirical observation (Buss, 1994; Singh, 1993; Thornhill, 1993). The optimistic voice of evolutionary psychology tells us that there are evolutionary explanations for all important psychological processes.

While there may be genes in everyone's theoretical future, there is growing concern that the adaptationist model is limited. The decomposition of human existence into specific adaptations may ultimately be indefensible (Gould & Lewontin, 1979). Evolution is probably much more complex than most theoreticians believe.

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Adaptive changes occur, but only within the limits of phylogenetic history. Complexity does not come from adaptations alone. Random drift of gene pools, environmental and physiological contingencies, emergent (epistatic) processes, allometric relations between structures and functions, self-organization of complexity, and developmental constraints play major roles in the molding of complex behaviors (Eldredge, 1995; Gould, 1966, 1977; Holland, 1995; Huxley, 1932; Kauffman, 1993; Raff, 1996; Schieve & Allen, 1982; Thiessen, 1997; Thiessen & Villarreal, 1998; Thompson, 1966). Evolutionary psychology has incorporated little of this theoretical complexity, and seems unlikely to move in these directions in the near future.

Linda Mealey (1999) of the University of Queensland shares some of these concerns:

> Evolutionary psychology, like human sociobiology before it, has a bad habit of "documenting" folk knowledge and providing post-hoc "Darwinian" interpretations of the "findings." While one of the major motivations for coinage of the phrase "evolutionary psychology" was to distinguish a new paradigm, different from the "just so" models of human sociobiology..., not many have taken up the challenge for a truly new approach and, so far, ... little has been documented that is truly new. Despite a few novel insights ..., evolutionary psychology has not lived up to its promise.

Moreover, evolutionary psychology is relatively disinterested in environmental interpretations of cognition and behavior, even though behavior geneticists have consistently shown that about 50 percent of phenotypic variation within populations is environmental in origin. Biologist David Barash (1997) adds that there is a lack of theoretical integration between the adaptationist perspective and the importance of individual genetic and environmental differences. Perhaps the apparent neglect of these areas during the early stages of its development is legitimate, but eventually evolutionary psychology will have to reconcile its enthusiasm for adaptationist models with non-adaptationist explanations of evolutionary change and traditional concerns for individual differences and environmental influences on human traits.

A possible resolution of some of these problems may be found

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by considering the ubiquitous influence of environmental context on the regulation of evolved adaptations. Just as S-R conditioning is regulated by the context in which it occurs, evolution of specific traits may be regulated by the surrounding environment in which the evolution occurs. The exact nature of this contextual imprint is unknown but could involve the imprinting of background cues along with the specific strategies for survival and reproduction. Whatever the mechanisms, adaptations may reflect their early context and generalize across environments according to their similarities to the original context. Thus the environments in which behaviors are expressed become critical regulators of behavior.

None of this argument calls for an abandonment of domain-specific hypotheses of evolution, but it does intensify the need for an expansion of the notion to include the mediating influence of background conditions on the formation and expression of specificity of function. The regulatory functions of the environment should take on greater significance, particularly with regard to non-reproductive and emotionally energized behavior. Parenthetically, the success of contextual evolution would help to further unite the goals and methodologies of evolutionary psychology and learning theory. In my thinking the severe form of the reductionistic model will only carry us so far; it is a great strategy for understanding the rough outlines of human behavior, but it cannot account for general regulators of behavior and cognition or their diverse and non-specific attributes that stem from the quality, intensity, and the frequency of surrounding environments.

Can Contextual Evolution be Experimentally Demonstrated?

A partial test of the contextual notion would be to artificially select a heterogeneous animal population for a predetermined trait level, as with high reproductive motivation in environments that differ in visual or other sensory characteristics, and subsequently test for these behaviors among *animals of selected lines who have had no reproductive experience in these environments*. One would first want to establish that the mating environment among experienced animals affects the strength of the response. To demonstrate this the animals would be given extensive sexual experience in one environment and subsequently tested in different environments. The mating behavior

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would be expected to deteriorate in novel environments, especially in those environments that are most unlike their initial mating environments. Once the importance of the environment is established as a modifier of the behavior, it becomes more likely that there will be co-selection of environmental context and mating responses.

There are some data in the learning literature to support the importance of original context in genetic selection (see Diamond, 1988). "Bright" and "Dull" rats seem to generalize their selected levels of performance to the degree that subsequent test mazes duplicated the original maze design. The general notion that environmental contingencies are integrated with Darwinian natural selection has been extended by Crawford and Anderson (1989). The designs can be explicitly modified to test the contextual model of evolution.

If the artificial selection experiment proposed here is successful, the *reproductive levels seen in naive animals* should correspond to the similarity of contextual cues with the original environment in which high sexual motivation was selected. While novelty effects on initial testing would be expected, the animals nevertheless should show higher levels of sexual performance in environments involved in the selection of their relatives. The following table presents the basic design and the expected outcomes.

The design excludes reproductive differences across selection environments, as the selection criteria are the same in all three initial environments. The design holds constant all environmental influences, in that no test animal has had earlier experiences in these environments. They only share genes in common with those animals who were under selection pressure. Finally, the design makes specific predictions about the outcome based on physical similarities between the selecting and testing environment.

If the results follow expectation they would clearly indicate that the context is a "co-evolved" feature of the selection, even though there was never the requirement that animals take into account the context in which the selection occurred. Subsequent experiments may determine the specific qualities of the environment that are inadvertently selected into the pattern of mating.

It is also predicted as a result of this selection that naive litter mates of the selected lines would demonstrate differential behavioral

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Table 2

Expected Sexual Motivations in Naive Litter mates Whose Relatives were Selected for High Levels of Sexual Motivation

Gradient of Contextual Similarity During Testing*	Testing Contexts of Naive Litter Mates Whose Relatives were Selected in the Following Environments		
	A	B	C
A	High	Medium	Low
B	Medium	High	Medium
C	Low	Medium	High

* The rank order, A, B, and C represent a continuum of contextual similarity.

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(emotional?) reactivity when placed individually in the different environments. This result would show that the context of the original selection imbues animals with reactions independent of the specific selection for reproductive success. The effects of the context would thereby be disassociated as a regulatory environment from that of the specific selection criteria. Obviously, the experiment is an attempt to show that the outcomes of selection are broader than predicted by a domain-specific model, and essentially follow the pattern found with contextual modification of S-R conditioning.

We Sense the Contextual History of Our Lives

Extrapolating from our discussion of contextual evolution I would like to expand the contextual view to include some of the deeper concerns of humans, speculative as they obviously are. It is probably true that our evolved history fixes the constraints of our existence and the specificity of many of our behavioral mechanisms, but within those constraints and in spite of the specificities we sense our deep-seated connectedness with all life and our personal touch with our phylogenetic history. We are more than ranting and raving mosaics of primitive adaptations, fighting for temporary ascendancy and then slipping back into oblivion. Evolution was not piecemeal or restricted to hominid adaptations. We not only adapted to the specific requirements of the African savanna – finding food, avoiding predators, destroying enemies, bonding with mates – we became a part of a global accommodation with the forces of the universe surging around us, the contexts of our selection. Our adaptations were broad as well as specific, linking us to the seasons, the life forms around us, and the earth beneath our feet – in short the biophilic contexts of our more specific forms of evolution.

In addition to our resonance to our biophilic past, we have a history of genetic affiliation with other species. Our evolutionary history is distributed across the entire animal kingdom where we share the genes and disposition of our most distant ancestors (Wilson, 1992). We are primate, in turn mammal, in turn vertebrate; we are the amalgamation of all of our ancestors, in ways sharing their experiences and melding those into our own. Their aspirations and pleasures lie at the base of our own; their fears are to a degree our fears. Perhaps in only fine taint we bring to our daily lives those

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processes that struggled through the Cambrian era, escaped the catastrophes of the Permian extinction, and flourished as the dinosaurs fell to their doom. But their taint must be there, imbedded in the genes that flow between species, offering us fragments of the knowledge of the past, setting forth a character that is enlarged beyond the specifics of our human corpus and linking us forever to the universal life process of the past.

We feel this connectedness with the physical world and with other species in our deepest nature, in our appreciation of beauty, in our sense of immortality, in our confrontation with our deepest fears, and in our striving for self-expression. The drums of evolution past beat out their universal rhythms, leading us to philosophy, poetry, meditation, and self-consciousness, and finally to spirituality. Biologist Stephen Kellert (1997) reflects on the beauty of this complexity by saying, "The recognition of an underlying web of connection in nature can foster a belief in a fundamental meaning and harmony governing human existence. Transcending the distinctiveness, separateness, and aloneness of the single individual, species, and moment in time, an ultimate unity and purpose is discerned. A picture of overall coherence emerges, even the possibility of some functional and meaningful end. Life is viewed from the vantage point of a basic and underlying integrity, harmony, even goodness of purpose." (p135) An understanding of our evolution may rest ultimately on knowledge of mechanisms, but that knowledge must be deeper than molecules, wider than individual consciousness, and more enriched than natural selection for adaptive traits.

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Menstrual Synchrony: A Methodological Comment

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A simple diagram shows how the synchronization process can cause equally numbered onsets to to become more different, even though the difference between equally numbered onsets is often used as a measure of syncronization. Comparison of identically numbered cycles has several other undesirable properties, such as the difference measure increases as the study goes on and sensitivity to numbering system. Tests of synchrony should compare the differences between the nearest cycles at the beginning of the study with those at the end of the study. Any differences between results obtained with this method and results obtained with the equivalently numbered cycles method tells something about how synchronization is achieved. Several studies, including those by Wilson et al, Trevathan et al, and McClintock appear to have used an inappropriate comparison of identically numbered cycles.

Key Words: Menstruation, menstrual synchrony, ovulation, pair-bonding.

Most studies of menstrual synchronization have compared equivalently numbered cycles (studies of synchrony have included Goldman & Schneider 1987; Graham 1991, Graham & McGrew, 1980, 1992; Jarett 1978, 1984[2]; Little et al. 1989; Matteo, 1987,

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[2] The Jarett (1984) article reports that "women did not significantly synchronize their cycles with those of their roommates, although there was a trend towards synchrony", while also reporting that "both psychosocial and biological factors were found to predict individual differences in menstrual synchrony", with no less than five traits being statistically significant. These two results appear inconsistent, since it is unusual to successfully predict individual differences in an effect that cannot be demonstrated to exist.

The Jarett article is drawn from her Ph.D. thesis (Jarett 1978). The article reports on the subject's synchronization as of January, which are only part of the results reported in the full thesis. Unfortunately, the results reported in the article and the results not mentioned are inconsistent, even though it was claimed many of the results were highly significant. Before doing her statistical tests, Jarett transformed her onset difference data, expressing it as a ratio of the September synchrony measure minus the December measure to the September (the initial month) measure, and then constraining these to exceed -1.

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McClintock, 1971; Quadagno, et al, 1981; Trevathan et al., 1993; Wilson, et al. 1991; A. Weller & L. Weller, 1992, 1993, 1995a, 1995b, submitted; L. Weller & A. Weller, 1993, 1997 Weller et al 1995). This method has major methodological problems that appear to be unappreciated. To provide concreteness the theoretical points will be illustrated with data from recently published studies.

One of the real problems in menstrual synchrony studies is that cycle lengths vary widely and that women frequently either miss a period.or have a very short or a very long cycle (as has been shown in the major studies of the cycle: Chiazz, Brayer, Macisco, Parker, & Duffy, 1968; Munster, Schmidt, & Helm, 1992; Vollman, 1977). How this natural variability is handled can affect the ability to detect any synchronization that is taking place, making the points made below of considerable practical importance.

McClintock (1971, p. 244) states that differences should be calculated over the same number of onsets in order to "minimize the chance coincidences that did not result from a trend towards synchrony". Most of the later researchers have followed this practice. The problem with the equivalently numbered cycles method can be illustrated by data from a recent paper (Trevathan et al. 1993). If the reader assumes a normal 30 day cycle, he would expect that the maximum difference between the menstrual onsets of any two normal women would be no more than half of that period, or 15 days.[3] However, inspection of Figure 1 and Table 1 in the above mentioned article shows numerous cases where the period between onsets exceeds this. The largest difference observed is -134 days, reported for the difference in the onsets of the fourth cycle in couple number 7. This very large difference suggests something odd is happening.

However, there is data to see what is happening. The first

Jarett used the Wilconxon test. The assumption behind the Wilconxon test are inconsistent with the use of data expressed in ratio form. This transformation probably explains the reporting synchrony. Thus her report of a trend towards synchrony is consistent with there being synchrony in her data.

[3] Looking at it another way, a date (one women's onset) between two dates (another women's onsets) can never be more distant from the nearest of the two dates than half the distance between the two dates.

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woman reports very long cycle lengths of 101, 67, and 60 days. By the onset marking the end of the third cycle she is at 228 days. The second woman has short cycles of 29, 27, and 28 days. By her third cycle she is at 94 days. Thus, the reported difference of 134 days for the two women's fourth cycles. However, it can be calculated that when the second woman is completing her third cycle at day 94, the first woman is completing first cycle at day 101. At this time, the two women are out of phase by only 7 days, a much more reasonable figure.

Their first case provides another illustration. Suppose the first day of S1's cycle is taken as day 0. From her tabulated cycle lengths, her next three periods would be on days 33, 63, and 86. The second woman's cycle apparently started 9 days later. From the data given on the length of her cycles, her periods were on days 46, 89, and 123. The women's third cycles end 37 days apart, as the authors report. However, since the first woman is ending her third cycle on day 86, and the second woman is ending her second cycle on day 89, the two women are only 3 days out of phase at that time. This is a small enough difference that the two women might even be menstruating at about the same time.

This 3 day difference seems a more useful measure of the difference in the two women's cycles, than the reported 37 day difference for equivalent cycles. Notice that with the measure that gives the 3 day difference, the two women's cycles have converged (since 3 days is less than the initial 9 days difference) even though the paper suggests their cycles have diverged (since 37 days exceeds the initial 9 days difference).

Hence with the traditional same cycle method (of simply measuring the differences between the onsets that are numbered the same), two women could be menstruating at the same time, but yet be shown as being many days out of synchrony. This happens whenever in the process of becoming synchronized the onsets occurring on the same days were associated with differently numbered cycles.

Missed periods provide a case where the numbered cycles method can easily fail to discover synchrony which is really there. Consider the extreme case of two women who are perfectly synchronized with 30 day cycles. There really is a mechanism that

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keeps their cycles linked. However, for some reason one woman misses what should have been her second cycle. What would have been the third cycle is now labeled the second cycle. The two women are now 30 days apart. There has been a 30 day increase in the synchronization measure, even though in all case when this woman menstruates, the other woman does so on the same day.

The proposal of this paper is that, in menstrual synchrony studies, an index case be picked for each pair, and that the minimum time from a specified point on her cycle (usually the onset) to the same point on the cycle of the other woman be used as the difference in the cycle phase.

A Graphical Example

Figure 1 illustrates the most obvious problem with merely comparing onsets that are numbered the same.

Initially, the top woman's cycle is almost completely out of phase with the second woman's cycle. However, by skipping half a cycle the two cycles are brought into what appears to be perfect synchrony.

Such an unusually long cycle could exist (see discussion later in the paper for why this is likely). Schank & McClintock (1992, p. 328) hypothesize that one of the mechanisms producing synchrony in rats is an unusually long cycle produced by a partial luteinizing hormone surge which is not strong enough to produce ovulation, but does reset the clock as if ovulation had occurred. They built a model incorporating this effect which accurately fits rat cycle data. If something similar to this occurs in humans, synchronization could be accomplished by the type of event shown in the figure.

Consider how the equivalent cycles method would handle the case shown. Since the peak of the cycle labeled B1 is slightly closer to the peak of the cycle labeled T1 than B2 is, the cycles would be labeled as indicated, with the initial difference being that between B1 and T1. In the equivalent cycles method, the cycles would be labeled as indicated and subsequent differences would be that between B2 and T2, B3 and T3, etc. As seen, the cycles are almost perfectly synchronized, but the lag between B2 and T2 is quite large, actually larger than the lag between B1 and T1. The synchronization process appears to have actually increased the difference between corresponding cycles. There is something wrong with a procedure

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which claims that such an impressive example of synchronization demonstrates a lack of synchrony.

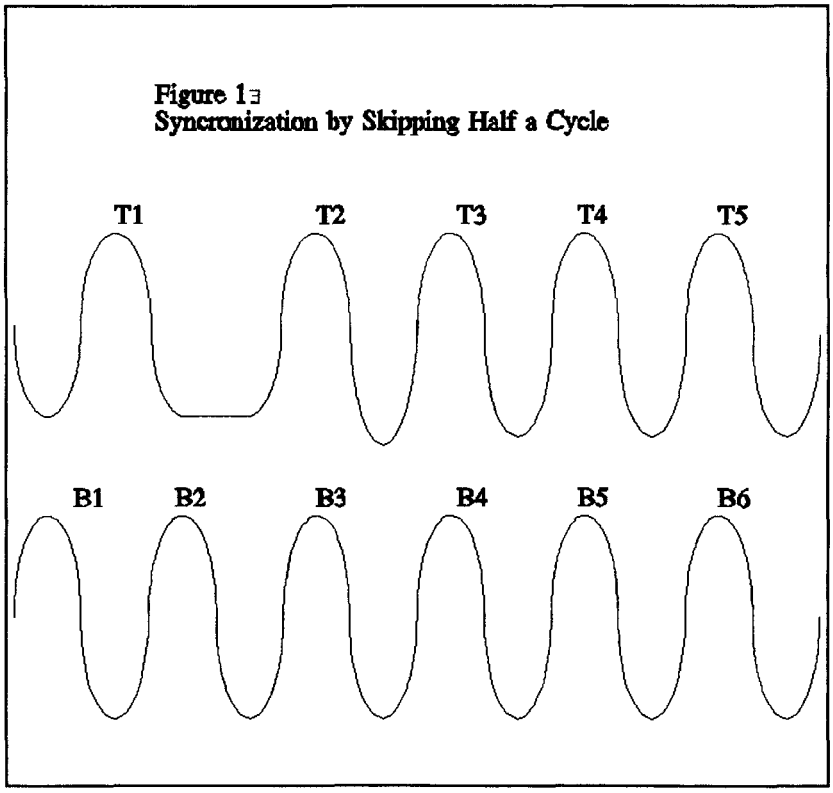

Figure 1∃
Syncronization by Skipping Half a Cycle

It might be noted that the equivalently numbered cycles method is very sensitive to where one starts numbering cycles. If B2 had been just a little earlier, it would have been labeled B1 (since the absolute difference between B1 and T1 would have been less than any other possible pairing of the first two onsets of each woman). If B2 had been B1 and B3 had been B2, the onsets difference would have been zero since onsets with the same number are compared, and synchrony would have been reported instead of lack of synchrony. A methodology in which a very small difference in the timing of one onset can have a large impact on the synchrony measure is incorrect.

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Likewise, if the scientist doing the study found the above pair after they had become synchronized, say after the third cycle of the bottom woman, the method for numbering the cycles would show no difference in onsets.

Interestingly, in this case where the difference in cycle phase is zero, the two women would be announced to be non-synchronized, since the definition of synchrony in the equivalently numbered system is that the difference in onsets must have decreased, and perfectly synchronized cycles such as in the right side of Figure 1 will be found to be non-synchronized by their method. The problem here is failure to draw the logical distinction between synchrony (a state)[4] and synchronization (a process increasing synchrony). By using the word synchrony when what is being discussed is the synchronization process, the paradoxical result is obtained that perfectly synchronized samples must be declared non-synchronous.

In the proposed system, the cycles might still be labeled for convenience as shown. However, one subject would be chosen. In the example, this is subject T (for top). For each cycle the time to the corresponding point (usually the onset of menstruation) on the nearest cycle of the partner would be determined. Thus for her first cycle, the entry would be the time between T1 and B1. For her second cycle the difference would be that between T2 and B3. A large reduction in the difference would be observed, and it would be concluded that synchrony had increased.

McClintock (1971, p. 244) states that onsets should be compared over the same number of onsets in order to "minimize the chance coincidences that did not result from a trend towards synchrony." However, in spite of what McClintock thought was desirable, her actual procedure does not guarantee that equivalent cycles were compared.

Wilson (1992, p. 583) has critiqued McClintock (1971) and others because their methods involved interviews at fixed intervals

[4] The strict dictionary definition of synchrony implies that events occur at exactly the same time. However, in the literature on menstrual synchrony, synchrony refers merely to a tendency for the events in the cycle to occur at about the same time more often than would occur in a random process.

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that probably picked up most cycles of most women, but could have easily missed a few of the shortest cycles. As a result, she was really assuming that the women had had a certain number of cycles rather than knowing for certain they had had that number (and probably some had had more). Suppose a woman had had seven cycles (one of which was not in the record) and the seventh cycle was close to that of the sixth cycle of another woman (all of whose cycles were recorded). Then, the difference between the seventh and sixth cycle would be assumed to be a comparison of two sixth cycles. If these were closer together than the first cycles for that pair of women, the pair would be reported as having synchronized. Thus, in the McClintock work a conceptual error is counterbalanced by a failure in implementation, which probably resulted in matching the two onsets that were closest.

Wilson (1992) and McClintock (1971), and probably most other researchers, seem to be mentally imagining that synchrony is gradually approached by reducing the time difference between onsets. However, as shown in figure 1, synchrony can be obtained by having an unusually long cycle, possibly by the mechanism Schank & McClintock (1992, p. 328) hypothesized. In this case, using a computational procedure that matches cycles can lead to error.

The Lack of Symmetry in the Equivalent Cycles Method

Logically the equivalent cycles method requires a way to pair the onsets to determine the initial onset difference, and hence to number the cycles. Wilson (1993) has proposed one such method. However, once a definition for the initial onset difference is adopted, a difficulty presents itself. Whatever method is used for defining the initial onset difference logically should also be used to define a closely related concept, the final onset difference. This is not done by the numbered cycles method, since the pairing that would result from using the same method used to pair the initial onsets does not necessarily pair the Nth onsets. There seems no logical reasons for defining the final onset difference in a different way than is used for the inital onset difference. If a similarly defined final onset difference is less than the initial onset difference, synchrony has increased. The answer given by the equivalent cycles method depends on how the onset to be labeled 0 is chosen. If a cycle for each woman at the end

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of the study should be labeled 0, the study cycles could then be labeled -5,-4,-3,-2, -1 etc. If the cycles are unsynchronized, the equivalent numbered cycles measure will be found to increase with time. Thus, the rule of considering an increase with time of the numbered cycles measure as evidence for a lack of synchronization appears incorrect. It is the result of an arbitrary assignment of the number 0 to certain onsets near the start of the study.

The High Weight Given to Non-synchronizers by the Numbered Cycles Method

When cycles have different lengths, about half of the time they will appear to converge for a while and then to diverge. In the other half of the cases, they will start to diverge and continue to diverge (Wilson, 1992).

A problem is that the divergence can quickly grow to large amounts for non-synchronizing pairs. Suppose the cycles start off not differing at all in starting date, but do differ in length by 5 days (say one is 27 days and the other is 32 days). They will diverge from day one. The onset of the sixth cycle will be 30 days apart, a very impressive difference. One should correctly conclude from this that this couple is not synchronized.

Suppose six couples started out five days apart, and after six cycles had onsets that were on the same day. The difference would have been reduced by 30 days. This 30-day reduction could equal the 30-day increase in cycle difference reported by a single couple that did not synchronize. In this (admittedly oversimplified) case the equivalent cycles methodology would report no change in synchrony, in spite of six out of seven women having perfectly synchronized. A simple comparison of the average differences in similarly numbered cycles could fail to discover a rather striking effect. (Admittedly, sign tests avoid this problem, but sign tests lose the information in the magnitude of the change).

This is an important problem. Even if all women have both the ability to emit and to receive the signals that lead to synchronization, it is very likely that various environmental conditions (presence of other women, stress, etc.) will prevent some women from

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synchronizing.

When comparing the dates of similarly numbered cycles, the relative weights of the pairs whose cycles have differing natural lengths, and which do not synchronize, increase steadily the longer the study. If the two cycles are off by 5 days, this adds a difference of 20 days after four months (the period McClintock proposed for most of synchronization to have occurred), 30 days after six months, 60 days after a year, and 120 days after two years. The result is that the impact of a few non-synchronizing women is greater the longer the study. If some women do synchronize, the chance of discovering the synchrony effect decreases steadily with the study's length.

It appears undesirable to use a study design with the property that the longer the study and the more data is collected, the lower the power of the study to detect the effect of interest.

The important thing is to realize that the equivalent cycles methodology typically shows a growing difference in onsets for any pair whose cycles do not influence one another. The inclusion of a few such women (which is to be expected given that McClintock reported that only some of the women in her study synchronized) can badly distort the average synchrony measure.

Discovering the Method of Synchronization

There are at least two ways that synchronization could occur, and at least two ways to measure synchrony. When the two methods give different answers, it says something about the synchronization process. The more obvious way is by the cycles staying roughly in phase, but the difference between the cycles steadily diminishing. This might happen if the first pair of cycles was 12 days apart, the next 10 days, then 8 etc. This seems to be the model behind Wilson's idea of first numbering the cycles, and then calculating the differences between comparably numbered cycles. It appears to also be the model behind McClintock's belief that menstrual synchrony is the best model as a system of "linked oscillators."

However, as pointed out, a cycle might be arbitrarily lengthened or shortened by what ever amount was needed to bring two cycles together (or to reduce the difference between them). As shown in the diagram this sudden process need not make comparably numbered cycles closer together. Biologically, this could occur by a signal from

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one female resetting the clock of the other person, or delaying a specific process. For instance, an odor derived from progesterone, if smelled (or received by the venomernasal system) when it was at peak sensitivity (which is around the time of ovulation) might slow follicular growth or delay ovulation. This could bring the cycles together. As discussed earlier, a method of measurement that is based on the time between one woman's onset date, and the nearest onset of the other woman, is correct if this is a possibility.

There are good reasons for believing that the changes in women's cycles that serve to bring them closer together occur in the follicular phase, or in the timing of ovulation. Vollman (1977) showed that there was a good correlation between the length of the post menstrual phase (the period from the onset of menstruation to ovulation as measured by body basal temperature, see figure 61. on p. 118) and the total cycle length, with an increase of one day in the post menstrual phase causing a one day increase in cycle length. This indicates that most of the variability in natural cycles is caused by the variation in the post menstrual phase (the follicular phase). In contrast, the pre-menstrual phase varied little with the cycle length (Fig. 63). Similar conclusions were found when the halves of the cycle were measured by intermenstrual pain (believed to be caused by the movement of the ova down the Fallopian tubes) (p. 158), and when measured by the cervical mucorrhea (p. 180).

It is very likely that part of the observed variation in menstrual cycle length is caused by the influence of other women, and this suggests that their influence acts on the post menstrual phase. This makes biological sense. The period from ovulation to menstruation is presumably controlled by the time needed for the body to determine that conception has not occurred, and to remove the lining of the uterus. If the purpose of menstruation is protection from disease as Profet (1993) has argued, a woman who slowed this process would increase her risk.

However, Cutler (1991) has argued that the luteal phase varies in length, and is regularized by pheromones from men. If the same pheromones are emitted by women, even if in smaller amounts (as is likely), it would follow that women could have at least a small affect on each other's cycles by this route.

Presumably, the purpose of synchronizing women's cycles is to

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achieve ovulatory synchrony (probably to reduce competition from other women for male resources, see Knight, 1991, Miller 1996, Turke, 1988). This would most easily be done by delaying ovulation or slowing the growth of the ovium. This would achieve an adjustment as soon as practical. Very likely, slowing development is easier than accelerating it.

Two methods for synchronization have been discussed: gradual changes that bring the closest onsets closer together, and a synchronizing process that could slow (or accelerate) a cycle even if the effect was to make the similarly numbered cycles draw further apart. The method of comparing similarly numbered cycles picks up only the gradual drawing together of the same numbered phases. Another method (the Miller method) measures the decrease in differences between onsets regardless of the system adopted for numbering the cycles. The latter method picks up synchronization achieved by both routes. If a particular woman is found to be synchronizing by the Miller method, but not by the equivalent cycles method, it follows that the method of synchronization was other than by a gradual reduction in the differences between similarly numbered onsets. This could be a useful test for synchronization occurring by slowing the cycle, including resetting clocks by delaying ovulation.

Since accelerating events may be biologically difficult, the possibility that synchronization is achieved by delaying events (as in Figure 1) must be considered.

It would be interesting to know how the length of the cycle changes in the women that synchronize. If it is done by slowing follicular growth, or by delaying ovulation in the presence of a roommate's progesterone derived pheromone, the women who are synchronizing should have an unusually long cycle. Thus, we should find a difference in the cycle length between the women who appear to be synchronizing (i.e. those women whose n+1th onset was closer to that of the partner than their nth onset was the partner's) and those who do not appear to be synchronizing. Especially, if the synchronization was by slowing follicular growth or by delaying ovulation, there might be appreciably more unusually long cycles in the set of women who were synchronizing than in those who were not.

Recently, Stern & McClintock (1998) published the first

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evidence regarding the mechanism of synchronization. They collected underarm odors on cotton pads from 9 women in two stages of their cycle, and then placed extracts (alchohol) of these on the upper lip (under the nose) on 20 women in different phases of their cycles. They found that the follicular phases were shortened (compared to the receiving women's earlier cycles) in women who received extracts from other women who were in their follicular phase, while the follicular phase was lengthened for women who received extracts from the women who were in their ovulatory phases. The lengths of the menses phases and the luteal phases were not affected by the extracts.

A Case Study

To illustrate the problems that can occur with the equivalently numbered cycles method, it is useful to look at the results of a application of the method. Wilson, Hildebrandt, & Gravel (1991) report two samples on which seven different analyses are done. The goal was to determine whether friends and roommates in a sorority and a cooperative housing unit had cycles that synchronized during the study. In all seven, the number of non-synchronous pairs exceeds the number of synchronous pairs. There were 177 pairs analyzed, of which only 29 were reported to be synchronous. The definition of a synchronous pair was one where the onset difference (as measured by the equivalently numbered cycles) shrunk from the start to the end of the study.

In theory, one might expect that the difference between two cycles that were independent of each other would shrink in half the cases, and expand in about half the cases. For instance, Wilson (1992, p. 567) himself stated, "a probable one half of the pairs in a sample synchronize by chance." Their results are far from this.

One unaware of the definition of synchrony being used might conclude there was a powerful tendency for women's onsets to be timed so that they came to be further apart. Indeed there is, if one compares the dates of onsets assigned the same number. However, this is just a result of cycles differing in length.

An interesting measure reported in this study is the final mean onset difference. These are remarkably large, ranging from 21.98 days for roommates in a sorority to 39.93 days for women in the establishment group in a co-operative housing group. These values

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should immediately raise questions. As Wilson (1992) himself had pointed out, for two cycles of constant length, the mean difference should be a quarter of a cycle's length. For a 30 day cycle, this would be 7.5 days.

If this study had shown evidence of synchrony, it would indeed be evidence using the common sense definitions. However, the opposite is not true. Failure with their definition to show synchrony does not show that synchrony has not occurred. As Wilson (1992, p. 570) himself put it in his theoretical paper "the greater weight given the increasing (divergent) differences can lead to the retention of the null hypothsis when it should be rejected." One suspects that may have happened in this study (see evidence below).

It is not known what the equivalent cycles measures would have been if the studied women's cycles were truly independent of one another. Certainly, the fact that the final onset difference greatly exceeds the initial onset difference is inadequate to show that women's cycles do not affect each other. If it was known what the equivalent cycles measure would be if the women's cycles were non-synchronized, it would be possible to compare the observed values with these. If the women living together (or otherwise interacting) had values of the measure significantly different from those consistent with independence, it would provide evidence that the women's cycles were affecting each other.

Ideally, women in the sample would have been randomly paired and then measures of differences in equivalent numbered cycles calculated. For instance, the cycles of women on different campuses (getting the same age distribution and same racial, class etc. distribution) might be collected, the women randomly paired, and Wilson type measures calculated. If after the same period of time, the final onset difference in the sorority roommate sample was significantly less (statistically significantly less) than for randomly paired women, this would be evidence of synchronization. (For examples of random pairing being used to provide a control see Goldman & Schneider, 1987; Graham & McGrew, 1980, 1992)

One thing is known. If women's cycles are truly not affected by the cycles of other women, the expected measures for a synchrony statistic should not depend on how the women are paired with each other. This condition is not met by the data in the study being used

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as an example. The pledges and mentors (who were housed in different buildings) and apparently were not close friends had an onset difference of 37.16 days after 242 days. The 14 pledges and roommates had a difference of 33.71 days after the same period. There is a difference here that should be explained. A possible explanation is that the roommates experienced some synchronization. Likewise, the roommates had a measure after 150 days of 21.98 days, versus 24.83 for the close friends. This difference may indicate greater synchrony among roommates than close friends, and could reflect the fact that roommates spend more time together.

In a cooperative house where the study period was 243 days (almost equal to the 242 days for the sorority), 28 "establishment" pairs averaged 39.93 days, 21 "rebel" pairs averaged 28.1 days, ("rebel" and "establishment" refer to cliques) and 6 close friend pairs averaged 29.5 days. The average final measure for these individuals (all of whom appeared friends) was (28 *39.93 +21 * 28.1 + 6 * 29.5 days)/((28 +21 + 6 = 33.75 days, very close to the measure for pledges and roommates, who also spent much time together. It is less than the difference reported for women not living together. Alas, the study did not calculate the final onset difference for the group in the cooperative house that was in neither the establishment nor the rebel cliques, since these were reportedly not a cohesive group. However, it is just such a non-involved group that is needed for a control, if one is to know if close contact leads to more synchrony.

However, this study does provide evidence of synchrony. The 21 roommates with cycles between 24.5 days and 32.6 days had an initial onset difference of 5.38 days, and a final difference of 6.10 days. The authors conclude "No menstrual synchrony is evident in this manipulated sample including only those subjects having regular cycles" (p. 358). This is true only by their definition.

This is a case where "the dog did not bark". The very small difference between the beginning and ending period is actually evidence of synchronization. There is a full week difference in the cycle lengths still in the sample (25 days to 32 days). Carried over 150 days, or about 5 cycles for most women, there should have been an appreciable increase in the final mean onset difference. For instance, if the two member of the pair had cycles that differed in length by only 4 days, after 5 cycles, they would be 20 days apart in final onset

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on average (the actual difference depends on the starting date, but this is the average effect). The only way to be as close in final onset as these women were is for synchrony to have occurred in many of them. Indeed, 6 of the 21 met an equivalently numbered cycles criteria for synchrony, which appears a rather stringent criteria for five cycles. This appears to be evidence for synchrony. (However, it is not known how often randomly matched women would meet this criteria, which is the statistic really needed to know whether this is evidence for synchrony.)

Conclusions

As illustrated by Figure 1, it is possible for synchronization to occur without the difference between similarly numbered cycles decreasing. Comparison of identically numbered cycles has several undesirable properties such as the average difference of the couples that do not synchronize increasing as the study goes on, and eventually coming to dominate the results. Thus, measures of synchrony (or synchronization) that depend on comparing the difference between comparably numbered cycles should not be used in testing for synchronization. Instead, one member of the pair should be picked, and the difference between the onset of this woman, and the nearest onset of the comparison woman calculated.

Several studies including Trevathan et al. (1993) and Wilson, et al., (1991) appear to have used the inappropriate comparison of identically numbered cycles. The latter study contains results that suggest that the dates of onsets were not truly random, and a failure of the average difference for women with normal length cycles to increase as would be expected for non-synchronized cycles of varying lengths suggests that some synchronization was occurring, even though the title of the paper suggested that no evidence for synchronization had been found.

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Social Fatherhood as a Prophylactic Against Violent Behavior: An Empirical Analysis of the Kinder, Gentler Side of Freud's Oedipus Complex

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The preclusion of a social father, due to out-of-wediock births, within a community is strongly associated with the level of violent crime within that same community. Both U.S. and cross-cultural data are presented as examples. The association remains strong even when an index of poverty – rates of male unempioyment – are controlled. An argument for causation rather than mere correlation is presented. However, divorce, a second method of separating a child from a father, generates a very different societal mosaic. Divorce rates are not associated with violent crime rates. The argument is presented that, if a social, on-going fether is able to share the same residence with his young, developing son, the son is less likely to engage in violent behavior as an adult. Freud's construct of and analysis of the OediPus Complex is thereby given empirical support (only) along the dimension of male aggression.

KEY WORDS: Father-child relations, divorce, out-of-wediock births, violent crime, Freud, Oedipus Complex, male unemployment.

The generalized imagery of the (U.S.) father figure has become something of a kaleidoscope which has been repeatedly turned by academics and the literati within the last quarter century. (See Demos 1986, Griswold 1993, LaRossa 1997, LaRossa et al. 1991, Nash 1965, 1976 for historical perspectives on the U.S. father).

Until very recently, the social father was a given in virtually any and all societies (Hendrix 1996, Hewlett 1992, Lamb 1987, Mackey 1996, Malinowski 1927, Van den Berghe 1979). Two very distinct, antinomous interpretations of this given are available in relationship to the value or function of the current generation of fathers within the U.S. and within any other society with an industrialized-service

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oriented economy. First, it can be argued that prior fathers had served the dual roles of protector & provider which were essential to the survival of his wife and his children. However, current governmental protectors, viz. local police, state police, FBI, national guard, and the U.S. armed forces, have efficiently and successfully undertaken the role of protector. The husband/father, who is less well trained for this role, is not needed. Similarly, governmental agencies, through local, state, and federal programs, have made death from privation and malnutrition extremely unlikely. Hence, the father's role of provider can also be supplanted either by working mothers and/or by governmental agencies. The argument would finish with the conclusion that social fathers in an industrialized, service-oriented, information based economy are somewhere between supernumerary or optional.

On the other hand, the second interpretation argues that the sheer omnipresence of social fathers strongly infers important functions of fatherhood that transcend differences in economies, religions, political structures, ecologies or diets. This paper suggests the latter position has validity and further suggests that one such function of a father is the domestication of his sons.[1] As a corollary, it is also proffers that there is no government agency of program which can replace or supplant this function of the child's biological & social father (see Blankenhorn [1995] and Popenoe [1996] for similar discussion).

Fatherlessness and Violent Behavior within the Community

The U.S. and other countries e.g. Sweden, are currently undergoing a social experiment. To wit, can a society perform efficiently and competitively if it assumes that the social father is either optional or supernumerary? For several decades, other studies

[1] Although not developed in this article, an analogous value or function of the resident social and biological father vis-a-vis his daughter would be argued that he lowers the chances of promiscuity on the part of his daughter (Coney & Mackey 1997). Also, this survey has not attempted to explore the possibility of a genetic bias toward violent behavior amongst those who sire out-of-wedlock births.

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have documented the association between fatherlessness and unwanted behavior on the part of children (Anderson 1968, Bereczkei & Csanaky 1996, Blau & Blau 1982, Chilton & Markle 1972, Mischel 1961a, 1961b, Monahan 1972, Mosher 1969, Robins & Hill 1966, Stevenson & Black 1988, Wilson & Herrnstein 1985, cf Adams et al. 1984). This exercise will focus upon serious violent behavior.

The numbers

In 1993, approximately 31% of all live births in the U.S. were births out-of-wedlock. In addition, another 1,000,000 minor children were involved in a divorce situation (U.S. Bureau of the Census 1996). The large majority of these children of divorce will end up residing with the mother (Sack 1987, Sitarz 1990). In 1994, more children were living with neither parent 4% than with father-only 3% (69% were living with both parents and 24% were living with mother only). Thus, about half of all U.S. children born in the decade will reside with both parents from birth to maturity (18 years of age), and about half of all children will be separated from one parent or the other (usually the father) prior to the children reaching maturity.

A current and on-going U.S. experiment in social engineering has been de facto designed. In large numbers/proportions, U.S. fathers have been precluded from (through out-of-wedlock births) or separated from (through divorce) their children. Accordingly, the magnitude of any putative relationship between father presence/father absence and violent behavior within their community can be attempted. Out-of-wedlock births – father preclusion – will be examined first. The primary analysis will be restricted to the U.S., but a cross-cultural perspective will also be given. Then, divorce – father separation – will be examined second. Again, most, but not all, of the analysis will be of the U.S. data.

Out-of-wedlock births and violent behavior in the U.S.

Four indices will be examined vis-a-vis the U.S.: (1) violent behavior and out-of-wedlock births in one recent year (1993), (2) violent behavior and out-of-wedlock births in a block of years 1987-1993), (3) the relationship between violent crime and out-of-wedlock births across a generation (1975-1993), and (4) the relationship between out-of-wedlock births at one time interval and violent crime at a second, different, time interval (i.e. a time lag of 19

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years will be examined).

To give context to these data, an index of privation will also be examined. A candidate for a highly influential factor in the genesis of violent crime has been poverty. Hence, additional data on one such index of poverty – male unemployment – will be examined. Male unemployment will be analyzed from the same time intervals as violent behavior and out-of-wedlock births.

Definitions and Methods

The percentage of all births which were out-of-wedlock births and the rates of violent crime (number of murders, forcible rapes, robberies and aggravated assaults known to the police per 100,000 population) plus rates of male unemployment were taken from the Statistical Abstract of the United States (U.S. Bureau of the Census 1971-1996). It should be noted that (young) men are heavily over-represented in the commission of violent crime (Bartol 1995, Siegel 1992, U.S. Dept. of Justice 1993, cf Mann 1996). The data are coded by state (plus the District of Columbia). Accordingly, the out-of-wedlock births, violent crime rates, and rates of male unemployment either were compared across states plus D.C. or were compared across years for the aggregate U.S. (all available states plus D.C.).

Ecological Inference

A problem which this type of analysis faces is that of the relationship between aggregate data and individual data. Framed differently, aggregate data are not equipped to isolate behaviors of an individual, and, thus, inferences from an aggregate, an ecological unit, are generally inappropriate when directed at any individual (see Robinson 1950 for an early discussion of the problem, and see Borgatta & Jackson 1980, Goodman 1959, Hanushek, Jackson & Kain 1974, King 1997, Langbein & Lichtman 1978, Pedhazur 1982 for subsequent discussions plus partial solutions to the problem of relating aggregate data to individual behavior). Suffice it to say that the analysis below is not construed to specify how any one individual would or would not behave. The analysis below is content (i) to attempt to discover what – if any – behavior patterns are aligned with other behavior patterns and then (ii) to attempt to decipher the char-

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TABLE 1.

Correlations (r_p) across the 50 states & the District of Columbia between percent of all live births which are children born out-of-wedlock and rates of violent crime: 1987 - 1993 (U.S. Bureau of the Census 1989 - 1995)

Year	Correlations (r_p, df = 49)	Percentage of explained variance: $(r_p)^2$
1993	.796 *	63.4%
1992	.773 *	59.8%
1991	.796 *	63.4%
1990	.815 *	66.4%
1989	.813 *	66.1%
1988	.780 *	60.8%
1987	.757 *	57.3%
Mean	.790 *	62.4%
(sd)	.021	3.3%

* = $p < .001$

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acter of any found alignment.

Results: U.S. Out-of-wedlock Births and Violent Crime

(1) Violent crime and out-of-wedlock births (1993)

In 1993, the correlation – across states plus D.C. – between the percentage of out-of-wedlock births and violent crime rates was significant (r_p = .796; p < .001; n = 51). Nearly two-thirds of the differences in violent crime rates can be attributed to differences in percentages of out-of-wedlock births ($.796^2$ = .634 = 63.4%). See Table 1. To avoid isolating an aberrant year, a block of continuous years (1987-1993) is examined next.

(2) Out-of-wedlock births and violent crime: 1987-1993

For this interval, the average correlation between violent crime and out-of-wedlock births was significant (r_p = .790 [sd. = .021]; p < .001; n = 51). The mean explained variance was 62.4% ($.790^2$ = .624 = 62.4%). See Table 1. Hence, the one year of 1993 was not an aberrancy.

(3) Out-of-wedlock births and violent crime in the U.S. (1973-1993)

Taking the U.S. as a unit, from 1973 to 1993 (or 21 years or n = 21), the correlation between out-of-wedlock births and rates of violent crime was significant (r_p = .958; p < .001; n = 21). Over 90% ($.958^2$ = .918 = 91.8%) of the variability in rates of violent crime be explained by differential rates of out-of-wedlock births.

(4) A lag between out-of-wedlock births and violent crime rates

Beginning with the year 1970, the variations of the percentage of out-of-wedlock births across (available) states and D.C. (n = 39) were compiled. Nineteen years were added to 1970 to arrive at the year 1989. (Approximately, 30% of all arrests of males for violent crimes occur for males who are 19 years or younger [U.S. Dept.. of Justice 1995]). That is, males born out-of-wedlock in 1970 would be 19 years of age in 1989. The percentages of out-of-wedlock births per state & D.C. in 1970 were correlated with the rate of violent crime per state in 1989. The result was significant (r_p= .874; p < .001; n = 39). Again, to avoid isolating an aberrant year, a five year interval (1970-1974) for out-of-wedlock births and a five year interval

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(1989-1993) for violent crime were analyzed. The average correlation for the five year comparison was significant (r_p = .882 [sd = .014], p < .001). See Table 2. In addition, this figure of .882 is higher (t = 3.20; p < .01; 2-tailed; df = 36) than the mean correlation between the percentages of out-of-wedlock births and rates of violent crime from the interval of years (1987-1993); i.e. r_p = .790.

Out-of-wedlock Births and a Cross-cultural Perspective

Arguably, this connection between unwed mothers and violent crime may be a (epi)phenomenon which is highly specific to the U.S. mega-tribe, i.e. a quirk. Because of the possible idiosyncratic character of the linkage, unqualified generalizability from the U.S. to other communities would be an exercise in the swashbuckling. On the other hand, the U.S. may simply be an example of a more general trend, a variation upon a theme. To discover if the linkage between violent crime and births to unwed mothers was more particularistic to the cultural matrix of the U.S., or exemplified a more thematic cultural dynamic, two similar analyses was conducted across cultures.

The first such survey is restricted to Nordic countries. The second survey is more universal and spans the globe.

Nordic out-of-wedlock births and levels of assault

Percentages of out-of-wedlock births and rates of assault per 100,000 population 15 years or older (14 years for Norway) were available from Denmark, Finland, Norway and Sweden for the interval 1959-1990 (Nordic Statistical Secretariat 1960-1992). The correlation between percentage of births which were out-of-wedlock and assault were significant for all four nations (Denmark, [r_p = .970, p < .001, n = 23], Finland [r_p = .851, p < .001; n = 20], Norway [r_p = .973; p < .001, n = 23], and Sweden [r_p = .945 ; p < .001; n = 22]).

If an interval of 19 years is used to separate the level of out-of-wedlock births and rates of assault the correlations are still significant (the correlations [r_p] range from .889 to .949; p < .001; n = 13).

Murder and illegitimacy across cultures

Cross-cultural analyses are hampered by the problems of mean-

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TABLE 2.

Correlations (r_p) across states (38) & District of Columbia between percent of live births which were out-of-wedlock and rates of violent crime. Each out-of-wedlock year (1970 - 1974) is nineteen years prior to the compared year of violent crime rates (1989 - 1993) (U.S. Bureau of the Census 1972 - 1976, 1991-1995).

Year of out-of-wedlock birth	Year of violent crime rates	Correlation (r_p, df = 37)	Percentage of explained variance $(r_p)^2$
1974	1993	.901 *	81.2%
1973	1992	.894 *	79.9%
1972	1991	.871 *	75.9%
1971	1990	.870 *	75.7%
1970	1989	.874 *	76.4%
Mean		.882 *	77.8%
(sd)		.014	2.5%

* = $p < .001$

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ingful and comparable units of analysis. (See Ford [1961] and Levinson & Malone [1980] for a discussion on cultural units). Analyses on rates of crime are often problematic because of the lack of consonance among countries in the definitions and reporting of various crimes. However, one crime – murder – does seem to be universally accepted as a crime with reasonably concordant definitions (Archer & Gartner 1984, Daly & Wilson 1988). One may muse on the high validity of the level of death as a dependent variable. Accordingly, rates of murder in the U.S. (U.S. Bureau of the Census [1994], p. 181) were correlated with illegitimacy rates in the U.S. – across the 50 states & D.C. – and then were compared to similar figures generated by cross-cultural data (Smith-Morris 1991, United Nations 1992). Note that the United Nations defines illegitimacy in the following manner.

"Legitimate refers to persons born of parents who were married at the time of birth in accordance with the laws of the country or area.

Illegitimate refers to children of parents, who, according to national law, were not married at the time of birth, regardless of whether these children have been recognized or legitimized after birth" (United Nations, 1992, p. 104).

The U.S.

In 1992, the relationship – across the 50 states plus D.C. – between murder rates and illegitimacy rates was significant ($r_p = .819$; $p < .001$). As illegitimacy rates increased, so did murder rates. Approximately two-thirds ($.819^2 = .671 = 67.1\%$) of the variance in murder rates can be attributed to differences in illegitimacy rates. See Appendix I for the data by state. Of interest is the relationship between percentage of out-of-wedlock births in 1973 to the murder rate in 1992. The correlation (r_p) was .900 ($p < .001$; $n = 39$). Over 80% ($.900^2 = .810 = 81.0\%$) of the variance in the murder rates can be attributed to the variance in the percentage of out-of-wedlock births. Again, the knowledge of fatherlessness in 1973 was a superior marker ($t = 2.97$; $p < .01$; 2-tailed; $df = 36$) for 1992 violent crime – in this instance murder – than that of the 1992 level of fatherlessness: ($r_p = .819$). See Appendix I.

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Cross-cultural data

There were usable data from the U. N. sources for 44 countries in which both murder rates and illegitimacy rates were reported to the satisfaction of the United Nations. See Appendix I for data by country. The relationship was positive and significant (r_p = .443; p < .01; 2-tailed; df = 42).[2] Across cultures, as murder rates went up, so did the proportion of single-parent births. Nearly 20% ($.443^2$ = .196 = 19.6%) of the differences in murder rates could be attributed to differences in rates of illegitimacy. Note that murder rates and divorce rates were not related (n.s., n = 39).

Discussion

While the numbers certainly suggest a strong correlative relationship between out-of-wedlock births and violent crime (as measured here), the argument being made is that for a causal relationship. That is, fatherlessness is suggested to be one causal agent (of perhaps many) which leads to violent behavior on the part of the fatherless sons who have grown to maturity. Complementing the robust correlations, additional evidence to be presented below for a causal relationship includes: (1) the lack of relationship between poverty & violent crime and (2) the temporal lag between out-of-wedlock births & violent crime.

(1) Poverty vs. out-of-wedlock births

It could be and has been argued that violent crime is a result of poverty (see Freeman [1975] Smith & Krohn [1995] for examples and theory for just such an argument from two time frames, cf Gillespie 1975). This position will be examined. Poverty, as used here, is

[2] The two rates – for illegitimacy (6.7) and for murder (38.7) – were also available for the Philippines. However, the Philippines' murder rate of 38.7 was over 8.5 standard deviations (sd = 4.09) over the sample mean of 3.62. Accordingly, the Philippines was enough of an outlier to be excluded from the sample. If rankings were used to generate the correlation coefficient (rs) and if the Philippines is included in the sample, then the relationship between illegitimacy and murder rates is significant (r_s = .889, p < .01; 2-tailed, n = 45). If the Philippines is not included in the sample, the correlation, based on ranks, is still significant (r_s = .896; p < .01; 2-tailed, n = 44).

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indexed by the percent of male unemployment within a state/D.C. Again, because of the over-representation of males, rather than females, in the commission of violent crimes, male unemployment seems the more appropriate index. Although there is a significant relationship between rate of male unemployment and violent crime for the year 1993 ($r_p = .298$, $p < .05$; 2-tailed; $n = 51$), the correlation coefficient falls to .139 (n.s.) if the percent of all births which were out-of-wedlock births is partialled from the relationship between violent crime rates and rates of male unemployment. Conversely, when rates of male unemployment are partialled from the relationship between the percent of out-of-wedlock births and rates of violent crime, the correlation coefficient is still significant ($r_p = .778$; $p < .001$; $n = 51$). The explained variance in violent crimes rates – with knowledge of percent of out-of-wedlock births – is a robust *60.5%* ($.778^2 = .605 = 60.5\%$).

Furthermore, for the 1987-1993 time block, the average correlation between rates of violent crime and rates of male unemployment was not significant ($r_p = .185$ [sd = .105]; n.s.; $n = 51$). See Table 3. On the other hand, if the rates of male unemployment during the 1987-1993 interval were partialled from the relationship between rates of violent crime and percent of births which were out-of-wedlock, the relationship is still significant ($r_p = .776$ [sd = .021]; $p < .001$; $n = 51$). The argument that poverty (at least as indexed by male unemployment) causes violent crime is difficult to sustain.

(2) A time lag: 1970 out-of-wedlock births and 1989 violent crime rates

Although "causation" is primarily a philosophical concept and immune from proof, the argument can certainly be made that father's presence helps to tamp down – causally – violent behavioral tendencies in their sons who have grown to maturity. Although the correlations from the same time frame between violent crime and fatherlessness may leave room for legitimate debate on "correlation" versus "causality", the successful prediction of violent crime rates by indices of fatherlessness nineteen years earlier presents a different order of rigor.

In any litany of evidence which tries to convince an audience of "causation", a time factor is necessary, albeit not sufficient, for

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TABLE 3.

Correlations (i) between percent of children born out of wedlock & violent crime, (ii) between male unemployment rates & rates of violent crime and (iii) partialled correlations [a] between out-of-wedlock & violent crime rates (with male unemployment rates partialled) and [b] between violent crime rates (with out-of-wedlock births partialled) for the years 1987 - 1990 and 1991 - 1993. (U.S. Bureau of the Census 1988 - 1994)

CORRELATIONS	YEAR:								
	1993	1992	1991	1990[a]	1989	1988	1987	MEAN	(sd)
Correlation between out-of-wedlock births & rates of violent crime	.796**	.773**	.796**	.815**	.813**	.780**	.757**	790**	(.020)
(Correlations with rates of male unemployment partialled)	.778**	.750**	.786**	-	.809**	.778**	.756**	.776**	(.021)
Correlation between rates of male unemployment & rates of violent crime	.298*	.305*	.209	-	.147	.103	.048	.185	(.105)
(Correlations with out-of-wedlock births partialled)	.139	.119	n.a.	-	n.a.	n.a.	n.a	n.a.	n.a.

* $p < .05$; 2-tailed; ** $p < .001$.

[a] Rates of male unemployment, by state, were unavailable for 1990.

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suasion. That is, if "A" causes "B", then "A" must occur before "B" occurs. (See Russell [1945] for an extended discussion). And such is the case being presented here: the incidences of fatherlessness occurred nineteen years before the incidences of violent crime. It is useful to note that part of the argument being made here is that father absence is neither a necessary nor a sufficient pre-condition for their son's violent behavior. Father absence is best conceptualized as being a contributory causal variable (see Susser [1973] for discussion). In addition, it is useful to note that male unemployment is only erratically related to violent crime, and if the impact of fatherlessness is removed, any found relationship between male unemployment and violent crime dissipates rather completely. Accordingly, to the extent that male unemployment signifies poverty, poverty is not a thematic variable generating both fatherlessness and violent crime.

The case being presented here notes that the incidents of fatherlessness occurred nineteen years before the incidents of violent crime. The 1989-1993 rates of violent crime could have no effect on 1970-1974 levels of out-of-wedlock births. The only other nominally proffered candidate for a causal influence would be poverty. However, as detailed above, male unemployment – poverty – is only erratically related to violent crime, and, if the impact of fatherlessness is removed, then any found relationship between male unemployment and violent crime dissipates rather completely. But, if the impact of the rates of male unemployment is removed, the relationship between levels of out-of-wedlock births and rates of violent crime is still significant and robust.

Results: Divorce and Violent Crime in the U.S.

Divorce rates (number of divorces per 1,000 population) for 1993 were compared to the rates of violent crime in 1993. No evidence of a relationship was found (r_p = -.064; n.s.; n = 47). A similar finding of no relationship occurred for 1992 (r_p = .010; n.s.; n = 47).

A partial explanation for the lack of a found relationship – negative data – could be a function of the demographics themselves. First, the standard deviations are not large. For 1992, the standard deviation was 1.2 with a range of 2.7 - 7.1 (the mean was 5.0 divorces per 1000 population). For 1993, the standard deviation was 1.1 with a range of 2.8 - 7.7 (the mean was 4.7). The plurality of all divorces

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involved no minor children at all – approximately 45% of the divorces. The next highest number was one child (25%), and half of these children were females who are not likely to commit violent crimes under any circumstances. Accordingly, the restricted variability would make teasing out relationships more difficult. That is, there may be too much noise in the system to find the signal. Nonetheless, the total lack of a direction in the data, in the context of a strong correlation between violent crime and percent of out-of-wedlock births, suggests that, in fact, rates of violent crime and divorce rates are operating independently of each other.

When divorce rates, across the aggregate U.S., from 1973 to 1994 were compared to rates of violent crime from the same time interval, the results indicated no relationship (r_p = -.264; n.s.; n = 22). Again, this lack of a found relationship is in contrast with the strong correlation (r_p = .958; $p < .001$, n = 21) which was found between violent crime and percentage of births which were out-of-wedlock.

There was no relationship found between divorce rates and either rates of male unemployment (r_p = .328, n.s., n = 21) or the percentage of births which were out-of-wedlock (r_p = -.312, n.s., n = 21).

A cross-cultural survey of divorce rates and murder rates found no relationship between the two variables (r_p=.106, n.s.; n = 33).

Thus, divorce rates – a separation of a father from his children – are not predictive of violent crime rates. Therefore, it appears that there are two different profiles concerning the two main reasons for fatherlessness: divorce and out-of-wedlock births. The preclusion of a father – out-of-wedlock births – and the separation of a father from his children – divorce – represent distinct phenomena, and this distinction will be examined below.

Divorce versus Out-of-wedlock Births

The level of out-of-wedlock births within a community is predictive of the level of violent crime within that community. Divorce, however, is not predictive. Both phenomena represent the absence of a father from a child, but only one phenomenon allows predictability: out-of-wedlock births. The question the becomes: "Why? Why would out-of-wedlock births be predictive, but divorce would not be predictive?"

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TABLE 4.

Percentage of divorces by number of children and by status of petitioner: 1982-1986 (Mean number of divorces = 573,931) (National Center for Health Statistics 1989).

Number of children	Status of petitioner: Husband	Wife	Other	Total	Ratio of Wife to husband
None	35.5%	55.9%	8.6%	100.0%	1.6
sd	0.3%	0.9%	8.6%		
One	27.8%	64.8%	7.4%	100.0%	2.3
sd	0.5%	1.00%	0.5%		
Two	27.6%	64.7%	7.7%	100.0%	2.3
sd	0.7%	0.7%	0.6%		
Three or more	27.4%	65.7%	7.9%	100.0%	2.4
sd	0.7%	0.8%	0.6%		
All children	31.3%	60.2%	8.5%	100.0%	1.9
sd	0.4%	0.8%	0.5%		

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A potential, if partial, answer involves the following logic. Clearly, with very few exceptions, the biological fathers were not available to their developing children when their children were born out-of-wedlock (Blankenhorn 1995, Cutright 1986, Hanson, McLanahan, & Thomson 1996, Hawkins & Eggebeen 1991, Popenoe 1996) In a divorce situation, again with very few exceptions, the father had shared a home with his child for some interval of time, then – after the divorce – the father was unavailable to co-reside with his child. The anomaly deepens when divorce statistics cum children are examined.

Petitioners

All states plus D.C. currently have a "no-fault" divorce system in place. That is, if a spouse wants a divorce, then the divorce will occur. The wishes of the other spouse, the respondent, are legally irrelevant. The allocation of property and custody of children may be negotiable, but the divorce itself is not negotiable. It will occur.

Across virtually all categories of divorce, women dominate in the petitioning for divorce (National Center for Health Statistics 1996). See Table 4. (See Beijing Review 1995, Buckle et al. [1996], Dennis [1992], Hong & Ning [1992], Peters [1979], and Sullivan & Allen [1996] for similar trends in countries other than the U.S.). If children (1 or more) are involved in the divorce situation, the women dominate in petitioning by more than a two to one ratio.

More than half of the men's petitions involve a divorce with no minor children. As a contrast, more than half of the women's petitions do involve one or more minor children (National Center of Health Statistics 1989). See Table 5. Thus, if a child is added to a family, the father's tendencies to divorce decrease, and the mother's tendencies to divorce increase. Men seem more willing to leave a wife than to leave a child. And, even though judicial edicts have proclaimed gender neutrality de jure in terms of custody, the end result – de facto – is a bias toward giving the mother custody (Fox & Kelly 1995, McIntyre & Sussman 1995, Westfall 1994, Weyrauch & Katz 1983). As was mentioned above, more minor children live with neither parent than with the father-only. With exceptions, contact between non-custodial fathers and their children is generally low and is consistently lessened over time (Furstenberg et al. 1983,

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TABLE 5.

Percentage of divorces by number of children, by husband as petitioner, and by wife as petitioner separately, 1982-1986 (National Center for Health Statistics 1989).

Number of children	Status of petitioner: Husband	Wife	z- score	p < (2-tailed)[a]
None	51.3%	41.8%	64.7	.001
sd	0.87%	0.07%		
One	22.2%	26.8%	35.8	.001
sd	0.15%	0.12%		
Two	17.1%	20.7%	30.8	.001
sd	0.35%	0.84%		
Three or more	7.0%	8.7%	21.0	.001
sd	0.30%	0.34%		
Number not specified	2.4%	2.0%	9.4	.001
sd	0.67%	0.39%		
Total	100.0%	100.0%		
Mean number	170,003	353,191		

[a] Because of the large n's, even small differences in percentages will generate statistical significance. Hence, the direction of the patterns is more emphasized.

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Furstenberg et al. 1987, Seltzer 1991, and Dudley 1991). The contact is typically not zero, at least not initially, but daily, extended contact is by far the exception which profiles the rule. The custodial parent (usually the mother) controls any contact between the non-custodial parent (usually the father) and the child rather completely (Braver et al. 1991). The non-custodial parent is not legally guaranteed access to the child's home. Hence, "visitation rights" really means the child can visit the non-custodial parent only when the visit is away from the child's home. In sum, social fathers normally will not initiate leaving/deserting their own children. In the main, if they are separated from their children, the separation is due to divorce laws and dissatisfied wives. Framed a little differently, it is arguable that a man-to-child affiliative bond had been forged which the man is reluctant to sever (see Mackey 1996 for an extended argument). Such an affiliative bond is less likely to be developed in an out-of-wedlock birth.

The divorce: Age of father and age of sons

The median age of divorce for husbands has been between the age of 32 and 36 years since 1970 (U.S. Bureau of the Census 1996). The average number of children per divorce which involved one or more children is approximately 1.7 (U.S. Bureau of the Census 1996). Thus, men involved in divorce more tend to be middle-aged rather than teenagers, and sundered families which involve at least one child, often have more than one child.

National data on the age of the child at the time of divorce are difficult to locate. However, Morgan, Lye and Condran (1988), using the Current Population Survey data, did survey over 90,000 children from divorced families and coded by age of child. The following patterns were revealed.

From families with one son (only), 34.7% the divorces occurred when the son was one-year old, 23.2% occurred when the son was two years old, and 42.1% of the divorces occurred when the son was three years of age or older. See Table 6. Framed differently, 42.1% of these sons lived with a social father for at least the first three years of their existence. For families with two sons (only), 91.7% of the divorces occurred after the first born son was three years of age or older. See Table 6. For families with one daughter and one son

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TABLE 6.

Percentage of children involved in a divorce by age of child and by birth order at time of divorce (adapted from Morgan, Lye, & Condran [1988]).

Number and gender of children	Age of first born child (in years): 1	2	3 or more	Total	N
One boy	34.7%	23.2%	42.1%	100.0%	17,567
Two boys	0.7%	7.6%	91.7%	100.0%	10,350
Two children: mixed	0.5%	7.4%	92.1%	100.0%	19,277

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(only), 92.1% of the divorces occurred when the first born was three years or older. If approximately half of the first born were sons, then approximately 46% of the sons lived with a social father for at least their first three years of life and their concomitant socialization. See Table 6.

Synopsis

A boy born out-of-wedlock has relatively limited contact, if any contact at all, with his biological father. A boy born into a marriage, even if the marriage ends in divorce before his eighteenth birthday, has a reasonable opportunity (45%+) to reside with his biological father for three years or more. After the divorce, some contact is maintained even if the trend is to have progressively lessened contact.

A mother is far more likely to initiate the divorce than is the father. When at least one child is included in the family, men reduce their tendency to petition for a divorce (women increase their tendency to petition for a divorce). The data suggest that men are much more amenable to divorce a wife than to "divorce" a child.

A thesis

Two points are useful to be reiterated. First, violent crime is strongly aligned with the level of out-of-wedlock births in a community. Second, violent crime is not related at all to divorce rates in a community. To help explain these two points, the following thesis is offered.

The thesis presented here argues that the early presence of a biological & social father tamps down tendencies toward violent behavior on the part of his son grown-to-adulthood. To the extent that this thesis is accurate, a theoretical model would be useful to lend coherence or synthesis to the analyzed data. It is suggested here that Freud's formulation of the "Oedipus Complex" is a relatively advantageous benchmark from which to begin to construct such a theoretical model.

A Brief Primer on Freud's Oedipal Complex

Sigmund Freud was many things, but a dedicated empiricist he was not. His development of the Oedipus Complex was an amalgam of anecdotes, interviews, intuition, and insight. There were no

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"control" groups and no "experimental" groups. Statistical instruments were not wielded at all. Nevertheless, Freud seemed both pleased and proud of his discovery. He wrote: "I venture to assert that if psycho-analysis could boast of no other achievement than the discovery of the repressed Oedipus complex, that alone would give it a claim to be counted among the precious new acquisitions of mankind." (Freud 1949).

Freud did have some very definite views on the relationship between a young son's psyche and his father. For example, he wrote: "(The superego's) relation to the ego is not exhausted by the precept; 'You ought to be like this' (like your father). It also comprises the prohibition: 'You may not be like this (also like your father) – that is, you may not do all that he does; some things are his prerogative'. This double aspect of the ego-ideal derives from the facet that the ego-ideal had the task of repressing the Oedipus complex; indeed it is to that revolutionary event that it owes its existence." (Freud 1961/original 1923; emphasis in the original). Said a little differently, the conscience is "heir" to the Oedipus Complex. Freud hypothesized that the dynamics of the Oedipus Complex become pre-potent around the age of three years and, if all went well, finished at about five years of age. A job well done was envisioned to include an internalized masculine identity by the boy and an effective superego, i.e. an effective conscience.

Without commenting upon the entire scope of the Oedipus Complex, several aspects are germane and, for at least the first two aspects, are close to being self-evident. First, the growing son loves (and is loved by) his father. Second, the growing son is intimidated by his father. The son is small. The father is large. The father can and does control the son physically. If a course of action is contested between the smaller, weaker son and the larger, more powerful father, the father wins. Third, in the synthesis of love, intimidation, and inhibition, the young son's superego is crystallized: the boy develops a conscience. A conscience is an internal inhibition against behaving in ways that the son's socialization has instructed the son are wrong (translation: based on experience – the sons's experience – the large powerful father will react in a non-desired manner if inappropriate behavior is emitted). The son identifies with the father and adopts/internalizes a masculine orientation. In addition, the

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authority and influence of the father inhibits the young boy's behavior, and the early inhibition reverberates throughout the life of the boy-grown-to-adulthood. The son adopts for himself an internal inhibition against behaving badly. For purpose of this argument, violent behavior is viewed as behaving badly. Certainly, violent behavior within the family domain is invariably counter-productive and discouraged.

Framed in the paradigm of comparative psychology, the son has imprinted on the father. "Imprinting" tends to lead to behavior patterns which are fairly permanent, resistant to extinction, and difficult to reorganize after initial organization has occurred (Freedman & Gorman 1993, Hess 1973, Lorenz 1935, Tinbergen 1951, 1965, cf Rossi & Rossi 1990). Thus, as Freud had assumed, rather than demonstrated, the early experiences of the child would be expected to influence the child, and those influences would also be expected to last into and through his adulthood. The data presented here sustain Freud's assumption.

Accordingly, the argument is thus.
(i) A boy who is socialized with his co-resident father, even for just his first few years, is relatively inoculated against engaging in violent behavior. He has an added layer of self-inhibition. His conscience is more potent. (ii) A boy not socialized with his father is relatively more likely to exhibit violent behavior. He has less self-inhibition, and his conscience is a ply less potent.

Summary

In the U.S., as elsewhere, violent behavior (which is heavily over-represented by young men) is strongly correlated with the level of out-of-wedlock births in the community. Single parent births effectively preclude father and son contact. However, the level of divorce is not related to violent behavior in the community. Because divorce also separates fathers from their children, a potential anomaly occurs. Both out-of-wedlock births and divorce curtail father and son interaction, but only out-of-wedlock births were found to be related to violent behavior. To help resolve the anomaly, it is suggested that some early contact between a social father and his son creates within the son an inhibition against unacceptable behavior, of which violent behavior is an example. As mentioned in footnote 1, the possibility

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of a genetic correlation between violent behavior and out-of-wedlock parenthood has not been researched. Bearing this reservation in mind, the data would seem to provide empirical support for the dynamics of Freud's Oedipus Complex.

APPENDIX I.

Rates of illegitimacy and murder across the United States (n = 51) (U.S. Bureau of the Census 1993) and across nations (n = 44) (Smith-Morris 1990, United Nations 1992).

State	Rates of[1] illegitimacy	Murder[2] rates	Country	Rates of[1] illegitimacy	Murder[2] rates
Maine	25.3	1.7	Australia	15.5	4.2
New Hampshire	19.2	1.6	Austria	22.4	1.3
Vermont	23.4	2.1	Belgium	5.7	3.1
Massachusetts	25.9	3.6	Canada	16.9	2.2
Rhode Island	29.6	3.6	Denmark	43.0	1.25
Connecticut	28.7	5.1	Finland	16.4	1.1
New York	34.8	13.2	France	19.6	4.05
New Jersey	26.4	5.1	W. Germany	9.4	1.5
Pennsylvania	31.6	6.2	Greece	1.8	0.85
Ohio	31.6	6.6	Ireland	7.8	0.5
Indiana	29.5	8.2	Italy	4.4	1.5
Illinois	33.4	11.4	Japan	1.0	1.2
Michigan	26.8	9.9	Luxembourg	8.7	7.0
Wisconsin	26.1	4.4	Netherlands	8.3	1.2

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State	Rates of[1] illegitimacy	Murder[2] rates	Country	Rates of[1] illegitimacy	Murder2 rates
Minnesota	23.0	3.3	N. Zealand	24.9	2.9
Iowa	23.5	1.6	Norway	25.8	0.9
Missouri	31.5	10.5	Portugal	12.4	3.0
North Dakota	22.6	1.9	Spain	3.9	2.3
South Dakota	26.6	0.6	Sweden	46.4	1.7
Nebraska	22.6	4.2	Switzerland	5.6	0.9
Kansas	24.3	6.0	Unit. Kingdom	19.2	1.3
Delaware	32.6	4.6	Unit. States	21.0	8.6
Maryland	30.5	12.1	Hungary	9.2	2.3
Virginia	28.3	8.8	Yugoslavia	8.4	5.4
West Virginia	27.7	6.3	Brunei	0.4	1.9
North Carolina	31.3	10.6	Fiji	17.3	2.0
South Carolina	35.5	10.4	Hong Kong	5.5	1.2
Georgia	35.0	11.0	South Korea	0.5	1.3
Florida	34.2	9.0	Sri Lanka	5.4	18.9
Kentucky	26.3	5.8	Mauritius	26.0	2.4
Tennessee	32.7	10.4	Cyprus	0.4	1.7
Alabama	32.6	11.0	Israel	1.0	1.7
Mississippi	42.9	12.2	Malta	1.2	1.8

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State	Rates of[1] illegitimacy	Murder[2] rates	Country	Rates of[1] illegitimacy	Murder[2] rates
Arkansas	31.0	10.8	Tunisia	0.3	0.7
Louisiana	40.2	17.4	Argentina	32.5	0.2
Oklahoma	28.4	6.5	Bahamas	62.1	12.2
Texas	17.5	12.7	Barbados	73.1	4.0
Montana	26.4	2.9	Chile	31.8	5.6
Idaho	18.3	3.5	Costa Rica	37.2	4.0
Wyoming	24.0	3.6	Jamaica	84.3	18.0
Colorado	23.8	6.2	Mexico	27.5	7.4
New Mexico	39.5	8.9	Panama	71.9	4.6
Arizona	36.2	8.1	Peru	42.6	1.2
Utah	15.1	3.0	Venezuela	53.9	8.4
Nevada	33.3	10.9			
Washington	25.3	5.0			
Oregon	27.0	4.7			
California	34.3	12.7			
Alaska	27.4	7.5			
Hawaii	26.2	3.6			
District of Columbia	66.9	75.2			

[1] Births to unmarried women, percent of total births.

[2] Offenses known to authorities per 100,00 population

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Intelligence and National Achievement

Editor

Raymond B. Cattell

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The Depiction of Herrnstein and Murray's *The Bell Curve* in the *PDR*

Daniel R. Vining, Jr.
University of Pennsylvania

This short paper records the author's reaction to a review of *The Bell Curve* that recently appeared in *Population and Development Review (PDR),* one of the leading demography journal in the world and the only one to have printed a review of *The Bell Curve.* This response, which was rejected by the *PDR,* comprises four parts: (1) the *PDR* reviewer's insistence that psychology is not a social science and therefore its discoveries can be disregarded, (2) the reviewer's (indeed all of social science's) hostile reaction to eugenics, (3) the reviewer's equally hostile reaction to *The Bell Curve*'s demography, and (4) the abandonment by the Population Association of America (PAA) of all study of the qualitative aspects of population, though such studies are firmly embedded in the PAA's constitution.

Key Words: Demography, population quality, IQ, eugenics.

The following is an extended comment on a review of *The Bell Curve*, which appeared in *Population and Development Review* (*PDR*). This comment was rejected by that journal, on the grounds that the *PDR* does not publish comments on book reviews. This was very unfortunate because the *PDR* is the only leading demography journal to have reviewed *The Bell Curve* and because there were numerous errors in this review that needed to be corrected. I am accordingly grateful to *The Mankind Quarterly* to allow my comment to be printed here. This way, some eyes will see it.

For those *Mankind Quarterly* readers who are not familiar with the *PDR* review of *The Bell Curve*, let me explain that the reviewer first summarized what he regarded as the five main points of the book, which he described as follows:

> "1. One's IQ is more important than one's early socioeconomic circumstances in fashioning a wide variety of adult outcomes (e.g., income, poverty, crime, illegitimacy).

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2. The economic payoff to high IQ has increased.
3. Occupational and educational sorting of IQ has grown, the American social structure is becoming increasingly rigid.
4. One's IQ is largely a function of one's genes; efforts to promote environmental betterment hold out little promise for improving the distribution of IQ in the population.
5. The mean IQ of the American population is about to decline, if it is not doing so already, because of fertility differentials by IQ and because of the immigration of persons who have, on average, lower IQs than natives."

Having made this summary, on which I will not comment, the review goes on to claim that the book does not support "this apocalyptic vision," and to present numerous criticisms many of which are in my opinion inaccurate or inappropriate. I was accordingly surprised when *PDR* rejected my letter of comment.

I accordingly reproduce below my letter to the editors of the *Population and Development Review*, which was rejected, together with some additional explanatory comments. My original submission is reproduced in Roman type, and the additional explanatory comments appear in italics. My submission really needs to be read in conjunction with the *PDR* review which it is criticizing, and although we cannot reproduce this here I hope that the interesated reader may consult the relative issue of *PDR* and consider my observations in conjunction with the review which I desired to criticize.

My Rejected Submission to *Population and Development Review*, Together with Some Explanatory Comments (These in italics)

In *Population and Development Review*, Vol. 21, No. 3, September 1995, pp. 675-677, Samuel H. Preston reviews Richard Herrnstein and Charles Murray's *The Bell Curve: Intelligence and Class Structure in American Life*. I wish to make some comments on that review here. This is imperative because Preston makes not a few errors in his review, errors which are probably common to demographers and others and should be corrected. Also, this is the only place where the *PDR* or any other American demography

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journal treats *The Bell Curve* in any kind of sustained manner.

[This paragraph is self-explanatory.]

In his first paragraph, Preston states that "[t]his variable [IQ] is rarely accorded much attention by the social sciences". If psychology, or parts of it, is a social science (even if it is not so deemed, for some technical reason, certainly demographers ought to be aware of its findings), then this statement is untrue. One finds quite a lot about "this variable" in that field. In addition, almost all the universities in the U.S.A. use SATs and GREs in their admissions processes, and these tests are simply IQ tests for late adolescents (see Allen [1992]). Since Sam Preston teaches at the University of Pennsylvania, one of those universities, he knows all about SATs and GREs.

[This paragraph, on the other hand, does require some explication. We are apparently in a statistically abstruse area. Heritability is fundamentally a statistical concept. However it is defined, it involves the variability about a mean. But that mean itself is autonomous. It can and does move in an independent manner. This fact is frequently misunderstood and ignored. The reviewer here ignores it, and our understanding is thereby not enhanced. In addition, human generation lengths are large; eugenic policies are, by definition, inter-generational. Much can and does go on within a generation. The reviewer is less than clear on both points.]

In his eighth paragraph, Preston writes as follows: "the policies required to avert the projected decline in test scores do depend on the outcome; eugenic policies are required if the genes dominate". It is unclear to me whether Preston here is paraphrasing *The Bell Curve* or expressing himself. Nevertheless, what he writes is untrue. In the first place, "eugenic policies" are very slow to take effect: 20 or 30 years. Generation lengths among humans are big, 20-30 years, and "eugenic policies" only take place across generations, by definition, never within. In the second place, even if there were 100% genetic heritability, that wouldn't mean that "eugenic policies" were called for. Heritability measures variation around a mean; it cannot and does not say anything about the mean itself. And yet we are frequently

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interested in movements of the mean. This is worth reiterating, because so many students and professors seem not to understand it: a high correlation between X and Y or a good regression of X on Y says nothing about the relationship of their means.

> *[This perhaps needs some clarification. Preston was not wrong in indicating that eugenic policies would counteract a decline in intelligence, although as I pointed out, they operate only in the long term. Where he was wrong was in in his statement that where "genes dominate" the ONLY solution is a eugenic one. Although it is true that genes are a dominant force in determining intelligence, since environmental factors are also relevant environmental intervention, such as improved nutrition, can raise intelligence. Preston is wrong in condemning The Bell Curve in this respect since the authors did not claim that intelligence was excluively determiend by heredity.]*

Paragraph 9 of Preston's review concerns the so-called "Flynn effect" and should be related to his paragraph 8. Flynn has shown that mean IQ has increased by about 15 points in 50 years, or about a standard deviation. But this is not unusual among biological variables: mean height, mean diameter of pelvic brim among human females, mean brain size have all gone up in the last 50 or so years whereas mean SATs, mean age at menarche, and mean sperm count (scientists are debating this), have fallen, all of which are due to environmental causes. Very rarely do we know what these environmental agents are. Preston seems befuddled in this paragraph.

> *[Here, the reviewer discusses the demography of The Bell Curve, which, at one point, veers into the reviewer's own work, which makes the astonishing claim that fertility differentials can be dysgenic but the mean eugenic. Do Herrnstein and Murray accept this "premise," as the reviewer rather desperately proclaims? Here are Herrnstein and Murray's own words: "the narrowest mathematical implication of their [the reviewer and a co-author's] model remains accurate: It is possible to postulate conditions that produce a negative or even rising IQ in the face of negative fertility*

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differentials. There is no reason to suppose that those special conditions prevail now or have in the recent past." (p. 735)]

In his last paragraph, where Preston discusses his own work (with Colin Campbell), he claims that "Herrnstein and Murray accept our underlying premise [that mean IQ can rise even when fertility differentials are negative with respect to IQ] in a long footnote". Do they? They don't, and that is why they relegate Preston and Campbell to a footnote.

[Here, I discuss the supreme irony that the Population Association of America's own constitution states as its goal the study of population in its qualitative aspect and yet never goes near the subject. This has led to a field, an important field, namely demography, being in a strong sense of denial.]

By the way, the goal of the Population Association of America (PAA), of which both Preston and the editor of this journal are past presidents, is defined in its constitution as the study of population in its "quantitative and qualitative aspects" (see, for example, any recent PAA Affairs [e.g., Le Clere 1995, p. 12]), and for a number of years the PAA's own Population Index classified articles under "policy on quality." The classification, however, is moribund and no longer appears in the Population Index's Table of Contents. So, we have a kind of relic word in the PAA's charter and *PDR*'s feeble efforts to suppress the subject of population quality in its pages. But can the subject of "population quality" be effectively held down in this way? I don't think so. Just the other day (January 29, 1996), *The Daily Pennsylvanian*, the student-run daily newspaper at the University of Pennsylvania, ran a front-page article on applications for admission for the class of 2000, writing as follows: "Applicants' average Scholastic Aptitude Test scores were slightly higher [than last year's], at 1,320 compared to last year's 1,318. The math score went up two points from 672 to 674 this year while the verbal score of 646 stayed the same. 'The quality hasn't been this good but that is something we say every year,' Admissions Officer Bill McCumber said." (Ahles 1996, p. 1) The only reason to avoid the subject of eugenics (the selective breeding of human beings) on the pages of this journal is

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the keen interest shown in it by the National Socialists of Germany 50-70 years ago and other more modern extreme right wing movements here and elsewhere (Vukowich 1971, *Harvard Law Review* 1981), the implied association with which in turn could harm one's career and standing in scholarly circles [hence, the hysterical title, "NeoNazis!" in a recent discussion of *The Bell Curve* in *The New Republic* [see Lane and Rosen {1994}]). But this is no longer a good or even respectable reason to avoid the subject of eugenics (many of the founders of the PAA were eugenicists, incidentally, sse Kiser [1981]) on these pages. If we accept Darwinism, then we have to accept eugenics (the application of Darwinism to human beings). The latter may be a dangerous idea, but so is atomic power and we hardly want to drive it underground, where some very strange ideas are lurking.

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The Mystery of the *Kalasa*: A Link to *Bharat Natayam*?

Charles E. Weber
American Numismatics Association

The author describes a 3rd/4th century A.D. Indian coin and suggests that the female figure portrayed on the reverse as standing by a *kalasa* or vase may provide a clue to the possible place of origin of the *Bharat Natayam*, a dance discussed by S. Soundarapandian in an earlier issue of *The Mankind Quarterly*.

Key Words: Numismatics, Yaudheya Gana, Bharat Natayam, India.

Amongst the most nicely executed bronze coins of ancient India are those of the Yaudheya Gana.[1] The coins seem to have been struck during the third or fourth century A.D. and are, fortunately, relatively common. Four of them in my collection weigh 11.17, 11.5, 11.27 and 10.7 grams, a rather remarkable consistency for bronze coins of that time. Such consistency of weight is strong evidence that these coins were dependent on their copper content for their monetary value and were thus not of a flat nature. The diameters of my four coins range from 22 to 25 millimeters.

The specimen weighing 11.17 grams has a diameter of 23 millimeters and can be described as follows:

Obverse: Standing, facing representation of the war god Karttikeya. In his right, extended hand he holds a spear nearly as long as he is tall. Under his left arm (also extended away from his body), that is, in the lower right field, is a peacock facing toward him. Around the perimeter is an inscription in Bramhi characters transliterated as *Yaudheyaganasya jayah* "Victory to the Yaudheya nation." Karttikeya wears an object on his head, perhaps a helmet.

[1] The Sanskrit term *gana* is cognate with Latin *genus*, and referred to a tribe or nation. The Yaudheya *gana* seems to have occupied an area on the upper plain of the Ganges, just east of the Land of the Five Rivers (the Punjab).

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Reverse: Karttikeya's wife is portrayed full length. Her right arm is bent and raised, while her left hand rests on her hip. In the left field there is a vase (*kalasa*) containing what appears to be a plant or plants represented by three lines. In the right field is a *triratna* (a religious symbol) over which there are three dots. The whole design is encircled by 27 rather large dots.

Enlargement of reverse
Original diameter: 23 mm.

Illustrations and information about the coin are to be found in John Allan, *A Catalogue of the Indian Coins in the British Museum*, 1936, reprinted in 1967, Plate XL, nos 2-6; Centennial Publication of the American Numismatic Society, New York, 1958, Plate L no. 5.; P. L. Gupta, *Coins* (in the series, India - The Land and People), New Delhi, 1969 (Bhadra 1891), Plate 10, no. 101.; E. J. Rapson, *The Coinage of Ancient and Medieval India*, Plate III, no. 14.; John M. Rosenfield, *The Dynastic Arts of the Kushans*, Berkeley, 1967 No. 286; and D. C. Sircar, *Studies in Indian Coins*, Delhi, 1968. Plate IV, no. 20.

The whole design follows somewhat the general pattern of the contemporary Kushan coinage, which, in turn, seems to have been influenced by the Roman coinage which circulated to some extent in India at that time. Sircar believes that the weight is on the 100-Rati standard. (The rati seed is reddish and about the size of a large barley seed.)

What is the significance of the flower vase? None of the references I have cited offers an explanation. However, an article by S. Soundarapandian of Madras University, entitled "The Origin of the

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Bharat Dance," published in *The Mankind Quarterly* in the Fall/Winter issue of 1993, pages 125-129, offers a tempting hypothesis for the presence of the flower vase as part of the design. Although Soundarapandian does not mention the Yaudheya coin type, his paper argued that the Bharat dance probably originated in northern India, possibly in Kashmir, having derived from an ancient folk dance, the Karahattam, which involved a pot or jar and was accompanied by "a calculated rhythm (*thalem*) generally believed to have originated in the second century B.C."

Soundarapandian enumerates ten classical principles of the dance, also known popularly as the *Bharat natayam*. One of the principles is that the head is not moved haphazardly. Our coin suggests not merely a walking motion of the wife of Karttikeya but a dance motion, since her right arm is bent and raised, while her left hand is on her hip, hardly positions which would suggest only walking or standing still.

In view of the technicalities of the dance, Soundarapandian observes that "head movements are restricted in Bharat dance. The dancer should not move her head as she likes. She should not tilt her head while doing foot work or other movements. Such restrictions suggest that Bharat dance is a developed form of a folk dance called Karahattam - a dance conducted with a *kumba*, pot, or jar, balanced on the head."

While the *kumba* (an earthenware or brass vessel filled with flowers or fruit, and decorated on the exterior) is balanced on the head of the dancer, she cannot move her head freely. If Bharat dance developed from Karahattam, this would explain why the same restrictions which control the movements of a woman balancing a jar on her head could have entered into Bharat dance.

The time and even the place where the coin was manufactured would render it possible that there is a connection between the illustration on the reverse of this coin and the early form of the Bharat dance. In light of the presence of the *kalasa* vase or jar, and the dance-like pose of the female figure, the design on the reverse of the Yaudheya coin type which we have described would seem to offer an intriguing parallel to the Karahattam dance. If it were in fact so linked, then the coin would constitute evidence supporting Soundarapandian's belief that Bharat dance originated in northern

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India, possibly Kashmir. From Kashmir the dance form would easily have spread to the Punjab and thence to the Upper Ganges valley where the Yaudheya Gana resided.

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BOOK REVIEWS

A Bicentennial Malthusian Essay:
Conservation, Population and the Indifference to Limits
John F. Rohe
Rhodes and Easton, 1997

In 1798, Thomas Robert Malthus wrote his renowned *Essay on the Principles of Population* in which he stated that ". . . the power of population is indefinitely greater than the power in the earth to produce subsistence for man." Two hundred years later, a far more congested world must make room for an additional 250,000 people every day (total births minus total deaths). Anticipating Darwin, in that the ability for living organisms to over-reproduce is the basis of natural selection and thereby of evolution, Malthus suggested that the pressure of human population on resources would eventually lead to a situation common to nature, whereby the level of population would ultimately be restricted by the carrying power of the globe. Those who do not think deeply have constantly rejected his reasoning, claiming that improved technology has increased food output. This, of course, is quite correct; but Malthus' detractors are then too often prone to assume that technology will continue to keep up with population increase. Questionable as this is, more seriously they also ignore the myriad ecological problems which the rocketing human population is creating. The problem is not simply producing enough food (and at the present rate of population increase the ability to achieve this is by no means guaranteed) but also the problem of waste disposal and of overburdening the ecosystem itself.

The fact that the seriousness of the population explosion is slowly coming to be recognized is mirrored by the background of this author – a person whose former experiences would not normally place him amongst the Malthusians. John F. Rohe is a mechanical engineer who was a Peace Corps Volunteer during the 1970s. Engineers have often shown too little concern for environmental problems, and Peace Corps workers sometimes tend to allow their compassion for the living to override concerns about the welfare of posterity. But Rohe does not appear to have fallen into either of these two traps. He is a man of compassion who has read the works

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of Garrett Hardin, that renowned prophet of the evils of overpopulation and shares the latter's concern for the future of humanity and the need to conserve the ecological system which nurtures it.

Author Rohe applies the logic of an engineer to analyzing the implications of overpopulation in his concern for future generations. Mankind, he points out, is already beginning to suffer the fate of any plant or animal population which experiences a demographic explosion that carries its numbers beyond the carrying-capacity of its environment. Several hundred thousand humans die of malnutrition every year, and violence and hostility are on the rise in the more over-populated regions.

Rohe revisits the principles expounded in Malthus' 1798 work in the light of the prevailing and ongoing population explosion throughout the Third World. This has been brought about by the scientific inventions and well-intended charity of the First world which has facilitated the continued high reproductive rate of the less advanced countries and reduced the infant mortality rate which formerly helped keep some form of natural balance between man and the rest of plant and animal life. Thus while the fertility of the Western nations has fallen below replacement level, that of the Third World has been boosted far above natural replacement levels to the point where the pressure of population on land and vital resources is such that vast regions are being deforested and rendered unproductive by erosion, even while we write this review.

Furthermore, Rohe warns, modern economic development is threatening the very ecosphere on which all advanced life depends, as the life-supporting potential of not only lakes and rivers but even of the ultimate well-spring of life, the oceans. Well-intentioned actions and misapplied charity that feed the onging human population explosion are destroying the vegetation, eroding the soil, contaminating the ground waters, and generally polluting the earth, the waters and the air. Meanwhile, individuals and statesmen seem to remain indifferent to the need for limits: their indifference "peers back at us" through the effects of the exploding human population.

Rohe quotes Garrett Hardin, who wrote about the need to conserve the air, land and sea as necesssary concomitants to life, common to us all, with telling effect, as in the following paragraph:

> Today, Dr. Hardin's metaphor of the "commons" retains its

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> relevance .... we encroach upon properties held in common. For example, smokestack emissions and groundwater contaminants intrude upon the absorptive capacity of air and water owned in common with everyone .... sharing the toxic burdens freely with all users of air and water. Air, water and other environmental "sinks" have become the modern day "commons." The "Tragedy of the Commons" is as relevant today as it was several centuries ago....

Indeed, the overriding message contained in Rohe's study is a chilling warning:

> Ours isn't the only species confronted with the prospect of excess reproduction. Natural forces cause species to make regular concessions to the carrying capacity of their ecosystem. The lemming is a small rodent. If they overpopulate the ecosystem, stress and disease sets in and their numbers will dwindle. All forms of plant and animal life capable of excess reproduction rub up against the carrying capacity of their environment.

And yet contemporary humanity thinks little of the fact that at the beginning of the present century the world population was only 1.6 billion, and that by the end of this century, just two years away, it will be 6 billion. Thus he concludes his warning that in a natural system population density often oscillates, but that human cultural achievements have changed the odds, possibly against us:

> In the 1950s, total [human] births exceeded total deaths every year by about 50 million ..... The 1980s added about 80 million annually. The third millennium could see us adding 100 million additional people every year .... Twenty doublings of the population will leave a mere .7 millionth acre of arable land for each person. Stephen Jay Gould sarcastically carries the absurdity still one step further: If human populations overshoot the carrying capacity of the planet and leave a degraded, polluted, irradiated and impoverished ecosystem, will we bounce back like the lynx [which suffered a severe contraction of population after overpopulation but narrowly survived extinction]? Or is it more likely that the system will be too hostile for human life? Will we "overshoot and oscillate" or is it our destiny to "overshoot and collapse"?

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Altruism and Beyond: An Economic Analysis of Transfers and Exchanges within Families and Groups
Oded Stark
Cambridge University Press, 1997

This book is the outcome of combining two historically separate areas of population biology. The formal models in *The Genetics of Altruism* continue the tradition of mathematical population genetics stemming from Wright, Haldane, and Fisher. But the substantive area to which these models are applied is the comparative evolutionary biology of social behavior, taking cases and data both from the social insects (the Hymenoptera and the termites) and from social vertebrates (chiefly mammals). Connecting these two fields the author seeks to produce a unifying framework of evolutionary theory relating altruism to cooperation on individual fitness.

The models developed are all evolutionary models involving reciprocity selection, kin selection, and group selection, each of which operates through mechanisms quite different from the other two.

The standard theories of kin selection are reevaluated, especially in social insect applications including the evolution of a division of labor. New conclusions are drawn to explain the circumstances under which formulas based on average coefficients of relationship will be applicable to studying kin selection outcomes. Axiomatic development of a new highly general class of sib selection models reveals numerous new evolutionary effects and possibilities. For example, "reversion to polymorphism" is one nonlinear effect through which excessively effective kin altruism may be unstable in evolution even though less effective altruism would be stable.

The authors believe that conclusions should be capable of qualitative statement, and substantial parts of this book are explicitly directed toward this goal. In particular, Chapters 1 and 12 are nonmathematical syntheses of the main findings, including the relationships between the present field and allied areas of population biology and social science (general evolutionary theory, ecology, demography, animal behavior studies, and the abstract analysis of social structure).

A Technical Appendix and Glossary provide the reader with necessary background definitions and concepts applicable to the relevant genetic theory.

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Crime as Destiny
A Study of Criminal Twins

Professor Dr. J. Lange

Departmental Director, Kaiser Wilhelm Institute

Translated by Charlotte Haldane

Foreword by J. B. S. Haldane

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